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Marmaduke T. Mendenhall

Legendo et studendo acquirimus scientiam.

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THE

PHILADELPHIA JOURNAL

OF THE

MEDICAL AND PHYSICAL SCIENCES.

SUPPORTED BY AN ASSOCIATION OF PHYSICIANS,

AND

EDITED BY N. CHAPMAN, M. D.

PROFESSOR OF THE INSTITUTES AND PRACTICE OF PHYSIC AND CLINICAL PRACTICE, IN THE UNIVERSITY OF PENNSYLVANIA.

> "In the four quarters of the globe, who reads an American book? or goes to an American play? or looks at an American picture or statue? *What does the world yet owe to American Physicians or Surgeons?*
>
> *Edinburgh Review, No. LXV.*

VOL. I.

PHILADELPHIA:
M. CAREY AND SON——CHESNUT STREET.
William Fry, Printer.
1820.

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Medical and Philosophical Intelligence, ...

PROSPECTUS.

NEXT to the invention of the art of printing, periodical publications probably exercise the most beneficial influence in awakening literary curiosity, and diffusing knowledge. Of their utility in these respects, we have conclusive evidence, in the extent of their multiplication, and the eagerness with which they are sought after and encouraged, in every enlightened country.

Deriving their materials from many and different sources, they are accommodated more readily than any other species of writing to the varieties of taste, and the several conditions of intellectual capacity, and improvement.

To the United States, productions of this nature would seem to be singularly adapted. An inquisitive and reading people, we are, however, so widely dispersed, as to render access, for the most part, exceedingly difficult to libraries, and other auxiliaries of learning.

To these inconveniencies, which the medical profession, in common with the rest of the community, experience, others, more peculiarly incidental to it, may be added. Engaged in the discharge of duties inconceivably oppressive, and with a penurious, and wholly inadequate reward, not a few of our practitioners have neither the means to collect, nor the leisure to peruse elaborate works, or to pursue with regularity, any course of systematic study. Expedients, therefore, cannot fail to prove acceptable, in their arduous and perplexing avocations, which are calculated, in any measure, to lessen expense, economise time, or abridge labour.

No scheme, in relation to these purposes, obviously
bines so many advantages, as a well conducted Jo
Being afforded at a moderate price, it comes within the
petency of every one, and conveys in a condensed s
over the widest expanse of country, the earliest intelli
of the discoveries and inprovements in science, penet
into places, from which more ponderous tomes, and lit
vehicles, are entirely excluded.

Nor in another view, is it scarcely less suited to th
isting condition of the profession. Destitute of libera
sure, and with too little of that sort of discipline so ess
to the greater and more complicated literary perform
we have the talent abundantly distributed, which exc
short and miscellaneous compositions.

The preceding considerations, have had no small
in the promotion of the present undertaking But weigh
they are, we confess the operation of other incitement
less cogent and impressive.

Ever since the establishment of our *Independence*,
become the habit of Europe, very wantonly to traduc
national character, our institutions, and achievements.
lumnies from this source, have been so long tacitly and
that they really seem now to be raised, as it were, t
the sanction of prescriptive privilege, and on each repet
to be marked by fresh acrimony and insolence.

Even allowing that we are as deficient as is alleg
literary and other polite attainments, it does not at all
tate against our pretensions to genius, or to generous
and dispositions. Candidly examined, our history will s
that in whatever course the energies of our people have
directed, there we are eminently distinguished.

It would be alien to the occasion, and not compatible
our limits, to prosecute, in any detail, the discussion o
very interesting subject. Enough, perhaps, will be con
ed, to answer our immediate object, in the simple affi
tion, that the more elegant occupations of the mind, ar
last to arrest the attention of a people, and as exper
teaches, have only flourished in the maturest state of

ety. Cast on a desolate shore, our first care was to provide for the proximate necessities of life, and next to lay those solid foundations, on which may be perceived through the vista of no distant futurity, to arise the most splendid edifice of national happiness, prosperity and glory.

Be the charge renewed, it may be more distinctly replied, that we are still in the state in which the useful is preferred to the ornamental, and that, as in the case of manufactures, being readily supplied from abroad, we have taken no pains to force the culture of literature or the fine arts by the warmth of individual patronage, or legislative provisions.

No part of the reproaches to which we have alluded, can, with a shadow of justice, be extended to the profession of medicine. Emphatically, we have done our duty. It may be safely said, that in no country is medicine, strictly defined, better understood, or more successfully practised than in the United States.

European physicians do surpass us, in classical education, and in variety, depth and extent of erudition. But in acuteness of penetration, and promptness of remedial resource,—in that species of tact, without which genius is cold, and knowledge inert,—the power whereby the means are accurately adapted to the end, and which in the treatment of disease confers vigour and efficiency, we are perhaps unrivalled.

Doubts as to the soundness of this position, will at once be dissipated, by reference to the periodical and other recent publications of Europe, in which may be traced doctrines and modes of practice, long prevalent among us, now eagerly adopted and very generally approved.

It is not difficult to assign some of the causes of this superiority. Necessity is the strongest incentive to exertion, and in all its tendencies, is original and inventive. It is the fate, as previously intimated, of a large proportion of those who toil at the profession among us, to be so situated, as to command few of the ordinary advantages, and hence they are driven mainly on their own resources.

Neither perverted by prejudice, nor enfeebled by any undue reverence for authority, the medical mind of the country was every where open to the reception of new impressions, when thirty years ago, the pestilence, which has since wasted our cities, made its appearance in a guise so anomalous and violent, as to render the existing principles of the science inapplicable, and to engage us intensely in a wide scope of observation and research.

As new lights were elicited, correspondent changes took place, and the spirit of reformation continuing to move on, eventually led to one of those revolutions, incident to the history of medicine, in which views were established, more pertinent to the condition of the disease of our climate, and in stricter conformity to the general advances of human knowledge, during a season of such active exertion.

It may be collected from the foregoing observations, that among our leading aims, will be to trace the progress of medicine in the United States, to vindicate our claims to certain improvements, to preserve these, as well as what may hereafter be done, from foreign usurpation, and lastly to evolve, and stimulate the genius of the country to invigorated efforts, by holding out a respectable and more permanent repository for its productions.

Connected intimately with one of the schools, from which has emanated a large share of these improvements, and where medicine is still most ardently cultivated, we shall have peculiar facilities in the execution of this part of our plan, though, at the same time, we are sensible, that much has been accomplished elsewhere, and is therefore to be drawn from other sources, and by the help of such as are friendly to our undertaking.

There is no section of the Union without able and intelligent medical men, or which does not present the amplest field for the display of talent, the extension of knowledge, and the consequent acquisition of fame. Confessedly, the indigenous medicinal vegetables have hitherto been imperfectly investigated, and much remains to be performed in relation to medical topography,—in the history of epidemics, or more common diseases, as modified by climate, the seasons, the state of weather, the habits of society, and other localities, and peculiar circumstances, appertaining to the infinite diversities of our widely spread territories.

Communications on these points, so curious in themselves, and of such high practical import, as well as on subjects, which more commonly enter into the constitution of miscellanies of this description, are earnestly solicited.

Nor do we confine our views exclusively to mere *technical medicine*. Contemplated in a shape so limited, whatever may be its boasted usefulness, our profession loses half its elevation and dignity.

The physical sciences, or at least the branches, having the closest affinity to medicine, shall share our attention. Exuberant in objects of curiosity, the United States most unquestionably afford the finest opportunities for the cultivation of this species of knowledge. Contributions, which tend to elucidate any part of our natural history, in the widest sense of the term, we shall always gladly receive.

Criticism will regularly occupy a part of our space. As our wish, however, is to instruct, and not to censure, or gratify our own vanity by wantonly wounding the sensibility of another, we shall restrict the exercise of it to those works only, which have indisputable claims to notice, by the value of their matter, or the merit of execution, and thereby be spared the pain of contending with unresisting feebleness. Native medical literature, by which we now mean, the art of elaborate writing, is still pretty much in its infancy, and we are persuaded may be more promoted by the language of tenderness and encouragement, than by any severity of animadversion, or harsh exposure of its defects.

Deviations from the rule of conduct we have prescribed to ourselves, will only be found in cases of obtrusive impertinence, or empirical pretensions, and under such circumstances, we shall ever be mindful of the legal maxim, "that the judge is condemned when the criminal escapes."

To complete our design, we mean at stated intervals, to give an analysis of the Foreign Journals, so copious as to embrace the most interesting of their contents, and to exhibit the progress abroad, of medicine, and the collateral branches of science. Distinct from the obvious advantage of such a synopsis, it is especially demanded by the cost and difficulty of procuring the works themselves. Extraordinary too, as it may appear, it can, perhaps, be here only executed with fidelity.

Controlled by hostile feelings, and the meanest jealousies, the most enlightened nations of Europe, and with whose literary proceedings we are chiefly conversant, perpetually offer proof of a mutual spirit of injustice, in the suppression or depreciation of each others merits, and more particularly, in relation to medical improvements. Too neutral in our position, to be warped or influenced by such considerations, we are in this case, the best prepared to institute a candid inquiry, and pronounce a just and impartial decision.

After so ample an exposition of our motives and views, we might, perhaps, be saved the declaration that in conducting this Journal no traces of local partialities, or sectional prejudices, shall be discerned. Characterized only by liberality, science turns with disgust from whatever partakes of narrowness and favouritism. We look to the profession at large, for countenance and support of a scheme that deeply concerns the whole, and which we are sensible, can only succeed by general approval and cordial co-operation.

An enterprise, such as we meditate, vigorously sustained by the various ability which we hope to enlist in its service, is eminently suited, in our deliberate opinion, to promote the best interest of medicine, and in the illustration of its character, to conduce in no slight degree to the moral ascendency and intellectual glory of the country.

THE

PHILADELPHIA JOURNAL

OF THE

MEDICAL AND PHYSICAL SCIENCES.

ART. I. *On the Nourishment of the Fœtus.*

By N. CHAPMAN, M. D.

NUMEROUS as are the speculations on this subject, they may all be considered as having reference to the one or the other of two sources.

1. That the fœtus is sustained by the liquor amnii received by the mouth.
2. That it derives its food through the umbilical vessels.*

---

* Besides these leading doctrines, one or two vague conjectures, scarcely deserving of notice, may be met with. Thus, aware probably of the impediments to the introduction of the liquor amnii into the stomach, Alcmæon, an obscure writer, hints, that the surface of the body imbibes it "like a sponge." Crude as this explanation is, it corresponds with the state of medical intelligence at the time. But at a more advanced stage of physiology, the hypothesis was presented in a shape of less absurdity by Levret, who maintains, that the fluid enters the system by a process somewhat analogous to cuticular absorption. Though thus reformed, the hypothesis never got into vogue, or acquired many supporters. It has seldom indeed been even noticed. Distinct from other objections, it would be easy to shew the impracticability of cuticular absorption in the fœtus, since its surface is covered by a coat of thick tenacious mucus, which must effectually prevent its taking place. But I shall not waste time in exposing its defects. That which no one defends, need not be attacked.

Democritus, as well as some of the other ancient authorities, Epicurus, Hippocrates, &c., aver that there sprout from the sides of the uterine cavity papillæ or nipples, to which the fœtus applies its mouth and sucks out a nutritive fluid. This sage conjecture is, in point of merit, about on a footing with the preceding.

In another place,* I have examined, very minutely, the grounds on which the first of these doctrines rest, and I think not without shewing that its chief, and perhaps its only claims to notice, proceed from the respectability of some of the names by which it has been patronised and supported.

Content with the argument contained in the paper to which I have alluded, I shall only now briefly state, that the liquor amnii does not answer the purpose imputed to it in the fœtal economy.

1. Because the fluid is not nutritious, being at every stage destitute of such properties, and, in the latter periods of gestation is always less pure, and often becomes acrid and exceedingly *putrid*, *feculent* and *bloody*.

2. Because it exists for the most part, in an inverse ratio, to the age of the fœtus, and its demands for nourishment, and, is sometimes totally deficient.

3. Because, the fœtus has been born in numerous instances, without any passage, by which the fluid could be introduced into the alimentary canal.

4. Because, prior to the expiration of the third month, the stomach and intestines of the fœtus are in a soft and pulpy state, totally unfit to receive or to perform their appropriate action on any alimentary substance.

It would seem, indeed, that during the growth of the fœtus, few if any organic functions are performed. The operation which goes on, almost exclusively, while it is in this state, is subservient to the perfection of its different parts. Till these ends are accomplished, all the organs, with one or two exceptions, continue quiescent and inactive. It is the heart and blood vessels alone, which, to any extent, execute their functions during uterine existence.

Whatever, therefore, may be the precise mode by which the fœtus is nourished, certain it is, that its own organic action is little concerned in the process. Of this, we can require no proof, in addition to what has already been mentioned, than that it continues to grow though destitute of of some of those organs, without which, life after birth could not be sustained even for a moment. Cases of undoubted authenticity are recorded, where fœtuses have attained to the full size, and in other respects were perfect in their conformation, in which the brain, or heart, or lungs, or one or more of the viscera of the abdomen were wanting.

---

* Eclectic Repertory, Vol. 2

The subsistence of the fœtus is parasitical. Its food derived from the parent, through the medium of the placenta, is in a condition to enter at once into its organization. By the previous elaboration which it undergoes, the aliment is wholly divested of its grosser and recrementitous matter, and it is on this account that the excretions of the fœtus are so small, or perhaps none at all.

Dismissing the one under review, without further comment, I am next to consider the second of the doctrines already mentioned, which is so plausible in itself, that it deservedly challenges our most serious attention.

This is of very remote antiquity, having been first taught in the school of the Stoics, and was generally adopted by the disciples of that memorable sect of philosophers. Entombed during the lapse of centuries, it was again revived about the era of the discovery of the circulation of the blood, and has since, with a slight variation, prevailed.*

At its restoration, and long afterwards, it was generally believed, that a direct vascular connexion exists between the fœtus and the parent, through the medium of the placenta. But the very reverse of this is at present most satisfactorily proved. It may truly be affirmed, that there is no point in anatomy, more indisputably settled. As such is the case, I shall not waste time by presenting any detailed demonstration of the fact. Yet it may be desirable to those, who are not, perhaps, well acquainted with the subject, to be put into possession of some of the leading objections to the hypothesis. I shall then state, in a few words, that the alleged vascular connexion is disproved, by the total failure of repeated attempts to detect it by the best conducted injections—by the want of correspondence in the pulsation of the umbilical cord, and the maternal arteries,—and by the essential difference which is observable in the fœtal and maternal blood. To these facts may be added, as a very weighty consideration, that if the circulation were really continuous, the blood of the mother would be introduced unchanged into the fœtus. Thus, we should have a species of transfusion of the blood of one individual into the vessels of the other, without any of those preparatory operations, required for the due assimilation of it to the peculiar condition and exigences of the recipient system. Mischievous as probably would be the effects in this case, still more so must they be, from the propulsion of the circulation by the mother's organs. Driven forward by the energies of her heart and arteries, the stream of blood urged in this way, could not fail, at once to crush and reduce to a chaotic mass, the exquisitely delicate and tender organization of the embrio, or even of the more advanced fœtus. Consequences so fatal, have therefore been guarded against by a provision of nature, which will presently be pointed out.

---

* We owe the credit of its revival to Laurentius, and Fabricius ab Aquapendente.

As the direct vascular connexion could not be maintained, another mode was resorted to, in order to explain the interesting connexion between the parent and her offspring. It was now insisted, that the uterine arteries deposit the maternal blood in the cells of the placenta, from which it is taken up by the radicles of the umbilical vein, and after being distributed through the fœtal system is returned by the umbilical arteries to the same cells, from which it is re-absorbed by the veins of the uterus, and conducted back into the maternal circulation.*

But the doctrine as amended, does not approach nearer the truth than in its primitive shape. The circulation of the cord and of the uterus is totally distinct. The placenta is composed of two parts, the one fœtal, and the other maternal. The first is made up of the ramifications of the umbilical vessels: the second, of those of the uterus, with the addition of intercellular texture. Though the vessels of the two are ultimately more or less blended, they never unite. This may be demonstrated in a placenta of the early months, by simple maceration, when the vessels of the respective portions can be pulled out of the cellular mass, without suffering the slightest laceration. Two plants growing near each other in a loose soil, with their roots much entangled, though not inosculating, present no bad image of the state and relation between the vessels of the placenta.

---

* Blumenbach's Physiology.

It hence follows, that the account given by the modern physiologists of the fœtal circulation is egregiously wrong. The effusion of blood by one set of vessels, and absorption by the other, is a creation of the imagination, and has no real existence. The arteries of the cord have no openings, but run on without any interruption of continuity, into returning veins. Those of the uterus, on the contrary, have an outlet or secretory duct, which pours into the cells of the maternal part of the placenta a fluid, which we shall presently see is destined for fœtal nourishment, while at the same time, the main current of blood passes on into the corresponding veins to be returned to the uterus. The one closely resembles the pulmonary circulation, and the other may be compared, though not exactly, to what takes place in the corpora cavernosa penis.

Be this however as it may, it must be conceded, that there is not the slightest intercourse by vascularity between the parent and the fœtus. That such is the case is evinced by a series of evidence, which, though diversified in its nature, is clear, concurrent, and irresistible.

It is proved by injections. Let the most penetrating or subtle substance, as crude mercury, or the spirit of turpentine, or size, be thrown into the umbilical artery, the whole of the matter will be returned by the umbilical vein. But if, on the contrary, we inject through the uterine arteries, the corresponding veins will be filled, as well as the cells of the maternal section of the placenta, while not a particle of it can be traced into the fœtal system. These experiments,* which have been so often repeated, with the same results, as to leave no doubts of their accuracy, are strongly supported by a collection of facts, too interesting to be overlooked, though perhaps they may by some be deemed superfluous.

It is well known, in the first place, that after the expulsion of the child, and the cord is cut asunder, no hæmorrhage ensues on the side of the mother, or rather the only blood lost is that contained in the fragment of the cord.

It appears, secondly, that the fœtus is not affected by the hæmorrhages of the parent: Cases are recorded, where the mother accidentally bleeding to death, a child vigorous and full *blooded* was born in the moment of her dissolution. Every practitioner has seen the same thing happen in the profuse floodings which sometimes attend parturition.

Nor is the converse less true. The mother is not affected by hæmorrhage from the fœtus. In the operation of embriulcia, by the laceration of the great vessels of the brain, nearly all the blood of the fœtus, amounting to a considerable quantity, escapes. But the woman, debilitated as she is by her previous sufferings, which are always severe under such circumstances, from the violence and protraction of the labour, does not seem to be more exhausted by this loss of blood, which she could not fail to be, were it detracted directly or indirectly from her system.

It has, thirdly, been of late discovered, that when, by a strong parturient pain, the placenta is simultaneously expelled with the fœtus, which occasionally occurs, the circulation in the cord will be maintained for some time, provided the child does not respire. But respiration taking place, the pulmonary circulation becomes established, and that of the placenta ceases. The very curious fact, which I have mentioned, was first noticed, about the same time, by Dr. Rousseau of this city, and myself. By placing the child and placenta in warm water, I have seen the circulation, in some instances, kept up for ten, fifteen, or twenty minutes. But the most remarkable case of this sort, occurred to two of my pupils,* where the period was actually protracted for upwards of one hour. Being *still* born, the child with its appendages, was immersed in a tepid bath, with a view to its resuscitation, and during the time stated, the umbilical circulation went on as actively, and uninterruptedly, as under the natural circumstances.

---

* Monro on Fœtal Nourishment.

Let this fact be admitted, and really, I can hardly see how it is to be disputed, and there is an end to all further discussion on the question before us. To my mind at least, nothing can be more complete and satisfactory. As it may, however, be viewed differently by others, I will proceed with the detail of some additional evidence. It is perfectly well known, that in all the lower animals, the monkey perhaps excepted, the placenta is not entirely deciduous. The maternal part is a permanent adherent growth of the uterus. The fœtal is caducous and shed at every birth. Now the separation of the two parts takes place without the slightest rupture of vessels, or loss of blood, which could not be so, were the circulation between the parent and fœtus, either continuous, or by effusion and re-absorption. Experiments with injections, indeed, shew that no such connexion exists in these cases.†

There are, moreover, some animals, in which the two sections of the placenta correspond so little, as to render it utterly impossible that they should be subservient to the same circulation. Thus, in the doe, the umbilical division is of a sanguineous complexion and highly vascular, while the uterine has a gelatinous appearance: and in the rabbit, the one part is red and replete with vessels, and the other white and ligamentous with little or no evident vascularity. To conclude this part of our inquiry, I shall now relate the substance of some experiments which, I think, go very far to confirm my speculations on this subject.

---

* Dr. Channing, now professor of Midwifery in the Boston Medical School, and Dr. Shelby of Tennessee. A similar case, in many respects, is recorded in one of the volumes of the Medical and Physical Journal of London.

† Monro on Fœtal Nourishment.

1. The side of a pregnant bitch was laid open, and the umbilical vein of the fœtal pups divided. As I anticipated, the hæmorrhage was profuse, and the fœtus, on inspection, nearly exhausted of blood. But in repeating the experiment, I previously tied the arteries of the cord, and little or no loss of blood was sustained.
2. By cutting the carotid arteries, I bled a pregnant bitch to death, and immediately afterwards, exposing the fœtuses, I found them all living, without apparently having suffered any diminution in the circulating fluids. The umbilical portion of the placenta, seemed also to contain the usual quantity of blood, while the uterine part was evidently empty and flaccid.
3. Knowing that madder deposits its colouring principle, I fed different animals while pregnant, on it, and though several parts of the maternal system was manifestly coloured, I never could detect the slightest appearance of it, either in the fœtus itself, the liquor amnii, or any portion of the secundines.

In the prosecution of the preceding experiments, I had the amplest opportunities of ascertaining, that there is really an essential difference between the fœtal and maternal blood. That of the former, is conspicuously more thin, has a less florid, or arterial complexion, and exhibits altogether, the character of crude and imperfect elaboration. By Bichat pretty nearly the same account is given of the fœtal blood, and if I be not deceived, it is somewhere stated by Fourcroy, that on chemical analysis, it presents different results.*

Two or three instances recorded by the celebrated Jenner, of the fœtus having been affected with small pox through the medium of the mother, are much relied on by some, as affording proof of a very intimate vascular connexion. Even admitting the premises, which I am not disposed to controvert, I do not perceive that the inference contended for, necessarily follows. The connexion in the case, as between parts of the same system, is maintained by sympathy, and in this way, the disease might have been imparted. Of all weak hypothesis, that surely is the most so, which would seek for an explanation in the conveyance of the contagion to the fœtus through the maternal circulation.

---

* By an Italian writer, Dr. F. Lavagna, of no mean repute, we are told "that the blood collected from the maternal portion of the placenta contains fibrin, and readily coagulates, while that of the umbilical cord entirely differs in these particulars." Vide London Medical and Physical Journal, for July 1819, p. 23.

Taking into view the whole of the evidence of the case, which I have presented, I cannot help thinking, that I am entitled to the conclusion, that the fœtus fabricates its own blood, and that the only dependence which it has on its mother, is for a supply of materials out of which it is formed. Does any thing more take place here than in the egg? Confessedly the chick, by its own organs, produces its blood, and what reason is there to doubt, that the fœtus, in the viviparous animal, which has an equivalent apparatus, is not competent to the same office?

By the renunciation of the doctrines which I have so strenuously combated, we are involved, at once, in the difficult inquiry as to the use of the placenta. To me, however, its function really does not appear so obscure, and is twofold according to my apprehension. It is certain, that the blood in passing through it, undergoes changes very similar to those effected by the lungs. The suggestion of the placenta acting as a respiratory organ, was originally thrown out by the celebrated Mayow. It was soon adopted by Sir Edward Hulse, court physician in the reign of Charles the Second, who gave it a more explicit shape. After this period, we lose sight of it, till not very long ago, it was taken up by Dr. Jeffrey* and Dr. French as the subject of their inaugural dissertations. Neither of these publications have I met with, but I learn that the doctrine is defended by the following arguments.

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* The present distinguished Professor of Anatomy in the University of Glasgow.

1. That the placenta resembles the pulmonary organs in its cellular texture and general conformation.

2. That the whole of the blood of the fœtus passes through it, as it does subsequently through the lungs.

3. That compression of the umbilical cord destroys life as speedily as compression of the trachea does after birth.

4. That some animals receive oxygen through a dense medium, such as the zoophytes, which are permanently fixed under water.

5. That the blood returning from the placenta, is proved by actual observation to be converted from a dark, venous, to a florid, arterial colour.

The last of these arguments, if confirmed, must be deemed conclusive. But in relation to the fact, some difference of opinion is entertained. By no small proportion of physiologists it is utterly denied, that any such change as is alleged, does take place in the fœtal circulation. It is, however, on the contrary, as positively affirmed by more than an equal weight of authority to be so conspicuous, as to admit of no doubt or dispute. By Jeffrey especially, it is described as "very florid," which is much too strong an expression. My own experiments have taught me, that there is an evident difference, though by no means so considerable, as afterwards exists. This, indeed, is what might be expected, by one who is conversant with the peculiarities of the fœtal economy. Deriving its heat chiefly, or perhaps exclusively, from the medium in which it is placed, as is shewn by that of a dead and living fœtus being the same, it requires none of those great actions, which, while they probably contribute to the evolution of animal temperature, give brighter tints to the blood.

It may here be reasonably demanded, whence comes the oxygen which produces the change in the recurrent blood of the fœtus? Difficult as this problem is, I cannot consider it as inexplicable. The analogy of the egg would instruct us, that there is a provision of the sort in the placenta.

As will be noticed hereafter more particularly, there is at the larger extremity of the egg, an accumulation of oxygen, which is from time to time replenished from the external atmosphere, as is proved by the chick perishing from obstructing the pores of the shell by smearing it over with oil or varnish. Nevertheless, I am inclined to suspect, that a different arrangement prevails with respect to the fœtus, and that it derives its oxygen directly from the maternal system, by the stronger affinity of its venous blood, perhaps, in the same manner as in the ovo-viviparous animals, for example, some of the serpents, which hatch their eggs within their bodies. Even allowing, however, that neither of these is the mode, and that, at present, we are unable to offer any satisfactory explanation, the doctrine is not, in the slightest degree, invalidated. We show that the effect takes place, and this is enough for its substantiation.

Besides oxygenating, or to speak more correctly, decarbonizing the blood, the placenta most probably secretes a fluid for the nourishment of the fœtus. Many of the earlier writers maintain this opinion, and, of its correctness, as regards several of the lower animals, no one at present doubts. It is attested by the highest authorities in physiology, and though in the human species it does not so clearly appear, we are by no means deficient in evidence of the existence of the function even in this case.

By Harvey, by Haller, by Blumenbach, by Saumarez, by Burns, not to swell the catalogue with inferior names, it is admitted as a matter of their own observation. It is termed by Harvey an "albumoid liquor." This fluid, which is most readily detected in animals, as the cow, &c. having a placenta constructed somewhat on the principle of the articulation of the ball and socket, may be seen soon after the death of the animal, oozing from the papillæ into the cotyledons.

In the human species, it is said to be secreted by the uterine arteries into the cells of the placenta, which is renderdered sufficiently probable, independent of the positive testimony to the fact, by the conformation of the parts. What other use, indeed, can be assigned to the placental cells, and to the outlets, formerly mentioned, of the uterine arteries?

Nor does the obscurity of this process altogether preclude an explanation of it. The consent between the uterus and mammæ, as well in health as in disease, is of so intimate a nature, that they may almost be considered as indissolubly associated. Coeval in their development, these two organs, in every subsequent stage, harmonize for the most part in their actions, and are singularly distinguished by simultaneous changes. But this is not invariably so, since we have several very conspicuous instances of their being alternately and oppositely affected.

Now without entering into any formal disquisition as to the cause of this striking sympathy, which indeed is obvious enough, from the similarity of the structure and functions of the parts, I shall only remark, that the connexion established by the anastomosis of the internal mammary and epigastric arteries,* affords one of the most perfect and beautiful solutions of the fact we are investigating, to be met with in the history of physiological speculations.

It was recently observed, that the actions of the uterus and mammæ, are sometimes alternate and opposed. The examples which I had more particularly in view at the time, are the suppression of the catamenia during lactation, and conversely, the interruption of the secretion of milk, so long as the former discharge recurs with regularity. Nor is this all: these organs often exchange functions. Thus, in a state of pregnancy, the office of forming milk, or at least a fluid closely resembling it, as nourishment for the fœtus, devolves on the uterus. But so soon as the child is born, this duty is transferred to the mammæ, the uterus relapses into a state of inactivity, for a season, and the epigastric artery, the immediate instrument by which the secretion is accomplished, becomes once more small, in comparison with its preceding size.† That the uterus is capable of this specific secretory office, is further shown, and perhaps still more clearly, by the well known fact, that in the cases, where the milk is suppressed, or, to use the more popular phrase, repelled from the breasts by cold or other causes, it is, by a translation of action, occasionally secreted, and discharged from the vagina.

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* Vide Blumenbach's Physiology.

† Ibid.

Nor is it less true, that when the milk does not come at the ordinary period after delivery, the lochia are much more abundant, and sometimes are not a little changed in colour and consistence, by the admixture of a lactescent fluid. These facts I consider as irresistible, and with them I dismiss this part of our inquiry.

By allowing the existence of this uterine secretion, we have very distinctly unfolded to us a source of fœtal nourishment. But the mode in which it is conveyed into the system, is a difficulty which still remains to be removed. By Harvey it was supposed to be absorbed by the radicles of the umbilical vein. This, however, cannot be the case, as it is clearly ascertained, that the vein executes a very different office. Besides, it is equally certain, that the power of absorption by veins, if possessed at all, is limited alone to blood.[*] To me it is manifest, that the fluid is taken up by a set of absorbents opening in the cells of the placenta, which run along the umbilical cord, and terminate in the liver, where, perhaps, it may undergo some changes which fit it more perfectly to enter into the circulation. That the liver answers this, or some other important end in the fœtal economy, appears highly probable from its prodigious size.

In advancing the preceding speculations, I am aware that they may be considered as wanting, in some degree, the support of established facts. It must be confessed, though anatomists have repeatedly asserted that absorbents enter into the composition of the umbilical cord, no one, perhaps, has hitherto satisfactorily demonstrated their existence.[†] May not this, however, in some degree be imputed to the want of minute researches? Confident that the fœtus is sustained by the maternal circulation, we have been careless to trace out any other means by which it might be accomplished. But are we, because absorbents have not been detected, utterly to deny their existence in the cord? This surely would be surrendering up our judgment to a most unwarrantable species of scepticism. The fact is, we have precisely the same species of evidence of their belonging here, as to several other portions of the animal structure. No one has disclosed the absorbents of the cartilages, bones, or of certain parts of the brain, and yet their existence is universally admitted. It is, indeed, not a little extraordinary that this should ever have been made a point of controversy. An absorbent is just as necessary a constituent of a living body, as a blood vessel. Neither growth nor reproduction can proceed without them, and they will always be found to coexist. Deprive the animal machine of either, and it must at once fall into ruins.

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[*] That a different language is held by the ingenious Majendie, I am aware. But I cannot perceive any thing in his experiments or reasonings, to support his conclusions. The confutation of venous absorption, by the Hunters, I think is perfectly satisfactory.

[†] By consulting the work on anatomy, of my friend the present Monro of Edinburgh, just published, I find that I have conceded too much on this

When we witness phenomena which require a particular agency, we conclude, though it may not be demonstrable to our senses, that it exists. The fœtus must receive its nourishment through the blood vessels of the cord, or by absorbents: there is no other way in which it can be conveyed. Having shown that it is not by the former, am I not entitled to infer, that it is by the latter?

It has sometimes been alleged that, granting the existence of the absorbents for which I contend, vessels so diminutive as these must necessarily be, cannot possibly answer the supposed purpose. But we should recollect, that as the fluid comes to the fœtus so fully elaborated, there is little or no excrementitious matter to be thrown off, and

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point. "Of the existence of absorbents in the cord," he says, "there can be no doubt, as several authors of credit, Meckel, Waltherus, Ludwig, Mascagni, Winsberg and Reuss, have described them." Vol. III. p. 449. It is further stated by Baillie, in his edition of Hunter's "Gravid Uteris," that the second Monro was accustomed to mention in his lectures, that he thought on one occasion, he detected lymphatics in the cord.

consequently a very small portion will subserve the ends of nutrition.

As the process of fœtal nourishment is well ascertained in the oviparous animals, let us see how far our hypothesis upon the subject will be borne out by the analogy. It is not my intention to describe in detail the complicated structure of the egg. This is not required, and might even obscure the explanation I am to offer. By my own experiments, which, on repetition, were confirmed by more than one of the graduates of this University, it appears that about the third day of incubation, the umbilical cord of the chick begins to pullulate and project forwards. At the expiration of the eighth day, it reaches the folliculus æris, or sac, placed at the larger extremity of the egg. The air contained in this reservoir, has been proved by the chemists to be pure oxygen. As in viviparous animals, the recurrent blood of the cord is found to be changed, and, I think, a little more heightened in colour. The vessels subservient to this circulation, are the only ones which issue from the umbilicus. None enter into the vitellus or albumen, though such are mentioned in all the histories of the egg which I have consulted. The vitellus would seem to be the food of the chick. It does not, however, reach the fœtal system through the cord, as is generally stated. This is done by a small tube, perhaps of the nature of a lacteal, which runs from the intestinum ilium into the substance of the vitellus, and is called from its discoverer, *ductus intestinalis Stenonis*. By this duct, the vitellus, now attenuated by the heat of incubation, under the influence of a vital power, is carried into the intestinal canal of the chick.

Notwithstanding what is so confidently asserted to the contrary, the albumen does not, in any way, contribute to fœtal nourishment, nor is it ever mixed with the vitellus. These two substances are inclosed in separate membranous coverings, which are entire, and admit of no communication in a natural state. The use of the albumen is the same as the liquor amnii; each fluid surrounds the fœtus, and gradually wastes as incubation, or pregnancy, advances.

By the vitellus, an ample stock of nourishment is afforded. There is even, indeed, a supply for the chick, immediately after birth, or till it is able to collect and digest its subsequent food. This residuary portion of the vitellus, which is usually of the size of a nutmeg, is sucked up, as it were, into the intestine, at the very moment that the chick is escaping from the shell. But it sometimes happens, that by a too sudden contraction of the umbilical ring, it is excluded, in which event, the chick inevitably perishes.

Now let us trace the parallel more precisely. It follows from what I have said, that in both instances, there is what is called the umbilical circulation, which conveys blood, as well from the viviparous as oviparous fœtus, of a dark and venous character, and returns it converted into arterial blood. The resemblance, therefore, in this respect is perfect. To complete the view, it remains for me only to show, that there is the same similitude in the source of nourishment.

As regards the egg, it is demonstrable that the vitellus answers this purpose, and that it is taken up and carried into the intestines of the chick, by a duct of the nature of a lacteal. Does not the same sort of provision obtain in the fœtus? We see in the placenta, an accumulation of a lactescent fluid, which must be destined for this end, and we presume that it is conducted into the fœtal body by absorbents, because, independently of the direct evidence, it cannot be done by any other agency.

On the whole, by comparing the process of fœtal nourishment, in these two classes of animals, we may discern the most striking similitude in every leading circumstance, and have afforded us, in scarcely a less degree, an illustration of the simplicity of nature's contrivance, as regards this interesting and important function.

It appears from a note in the last edition of Richerand's Physiology, that a German writer of the name of Schreder, entertains views of this subject very analogous to those I have advanced. Whether he has a priority of claim, I am unable to determine, as the date of his publication does not appear. I have taught the doctrine in my lectures since 1803, and read a paper on it at Edinburgh, two years before.

ART. II. The Double Canula and a Wire recommended in the operation of extirpating Scirrhous Tonsils, and Hemorrhoidal Tumours. By PHILIP SYNG PHYSICK, M. D.

MY object in the present communication is to recommend the employment of the double canula and a wire, for extirpating scirrhous tonsils, and hemorrhoidal tumours, in preference to any of the other means which have been used by surgeons in such cases. I am well aware that the double canula has been used for the removal of enlarged tonsils by others; but I apprehend that from the manner of its application and management, every advantage has not been obtained which the operator may derive from that instrument.

After the application of the instrument, it has hitherto been the custom to allow it to remain until the tumour was separated and thrown off, a process seldom completed in less than a week or ten days, and sometimes occupying a longer period.

During the whole of this time the patient's sufferings have generally been very severe, arising from several circumstances next to be mentioned.

And first.—From the presence of the canula in the mouth, resting on the edge of the tongue, and passing out at the corner of the mouth, great irritation is given to these parts, sometimes occasioning ulceration; and I have seen the chin also rendered sore, by the constant flow of saliva over it, passing by the sides of the canula involuntarily.

Secondly.—From the compression of the wire at the base of the tumour, on the soft parts, being continued while they are inflaming and swelling. This ligature increases the tension and pain very much, as may be readily understood by any one who reflects for a moment on the effect of any ligature applied over an inflamation in any other part of the body. In addition to the pain, a high degree of symptomatic fever sometimes supervenes, attended with great restlessness and want of sleep, and in one instance which came under my notice, with delirium.

Thirdly.—From the instrument hanging out of the mouth, irritating by its weight. It is likewise constantly liable to accidents in wiping the mouth and lips, and when touched, never fails to occasion great pain.

Fourthly.—From swallowing being rendered so difficult and painful that the patient is very unwilling to attempt it. When it is effected, great pain is always experienced.

Having observed the above inconveniences, not only in my own practice but in that of others, I tried excision with scissors or a knife, and the application of caustic; but these operations being attended with many difficulties, the following method of using the double canula occurred to me many years ago, and I have since found it to answer the purpose very satisfactorily in every instance. The double canula I employ is about four inches long, with short arms soldered on the sides, near one end of the instrument, at right angles to it. Through the canula I next pass a doubled iron wire, and fasten one of its extremities round one of the arms of the instrument, leaving the other free and projecting five or six inches. This enables me to increase or diminish the size of the noose, formed by the doubling of the wire, at pleasure. The selection of a proper piece of wire I consider of much importance. It should be tough and flexible, formed of soft pure iron, having firmness enough to allow of its being pushed backwards and forwards in the canula, without bending too easily, so that the noose may be enlarged or diminished. It should also have sufficient firmness to allow of a little lateral pressure, otherwise the noose cannot be pressed down so certainly on the base of the tumour. The wire I use is about one twenty-fourth of an inch in diameter, or perhaps rather less.

It is moreover necessary to be provided with a pair of flat pliers, to take hold of and move the wire conveniently.

These instruments being prepared, the noose formed by the doubling of the wire projecting beyond the end of the instrument, is to be made large enough to pass easily over the enlarged tonsil, and should be bent a little to one side, in order that it may more easily be pushed down upon the base of the tumour.

The patient is to be seated opposite a window, and his tongue must be held down by an assistant, with the handle of a large spoon, or with a spatula. The surgeon is then to slip the noose over the tonsil, and down to its base, taking care not to include the uvula, which, when the swelling is large, is apt to be in the way. The wire is then to be drawn sufficiently to fix it loosely on the part, and the surgeon is to satisfy himself, by an attentive inspection, that it is properly applied. This being accomplished, the wire is to be taken hold of with the pliers, and drawn through one side of the canula, so as to secure it at once on the base of the tonsil as firmly as possible, and then to fasten it on the arm of the instrument, and thereby prevent all entrance of fresh blood into the tumour. This method of stopping the circulation of blood in the swelling, necessarily occasions severe pain at the moment; but the severity of it soon ceases.

On examining the tonsil after a few minutes, its colour will be observed to be changed to a deep purple or almost black, and its surface smooth and polished, owing to the exterior membrane being stretched.

It has hitherto been my custom to allow the instrument to remain thus applied for twenty-four hours, with the view of destroying completely the life of the enlarged gland. I am, however, of opinion, that a much shorter time would be sufficient, as eight or twelve hours, which I propose soon to ascertain. After having destroyed the life of the swelling by the above means, the next step of the operation is the removal of the instrument, which is very easily accomplished, in the following manner. Take a firm hold of the end of the canula projecting from the mouth, then disengage the wire on one side from the arm of the instrument; straighten it, and with the pliers push a small portion of it back through the canula, and repeat this until the noose is so much enlarged as to slip off the tonsil.

The operation is now completed; the tumour appears shrivelled and of a dull white colour; the patient suffers no pain; the inflammation is moderate, and, after a few days, the dead parts are separated and thrown off, either entire or in fragments, which are sometimes spit out, sometimess wallowed. Until the separation is completed the breath is somewhat offensive. I have never had any trouble with the small ulcer remaining after the separation of the tumour. It has healed so rapidly as generally to escape notice.

I have for many years been in the habit of performing the same kind of operation for the extirpation of hemorrhoidal tumours. The canula used in this case, should not be longer than about two inches. When hemorrhoidal tumours are external and troublesome to the patient, almost all surgeons, I believe, cut them off; but when their attachments are within the anus, and the tumour only protrudes in the act of evacuating the fæces, then their excision would be attended with great risk of hemorrhage. This some have denied; but having twice witnessed the fact to a very alarming extent, I wish on all such occasions to guard against it. The extirpation of such tumours can be performed safely, by means of a ligature of either vegetable or animal substance; but the most convenient and effectual I have ever tried, is a wire drawn at once tight round its base, by means of the double canula. This gives momentary pain; but it is not in all cases so severe as might be supposed. I am not able to account for this circumstance; but some patients make no complaint whatever, even though two or three tumours are operated on at the same time, while others exclaim violently from its intensity. At the end of twenty four hours, and probably sooner, the wire may be removed in the manner above explained. The tumour will be found shrivelled and black, and in a few days will be separated and thrown off, under the application of a soft poultice of bread and milk.

As soon as the wire is removed, the patient is relieved from almost all uneasy sensations, and the ulcer heals very readily, as after the extirpation of the tonsil. No one can properly appreciate the advantages resulting from the above method of removing hemorrhoidal tumours, who has not seen them treated by allowing the ligature to remain during the separation of the part. Under that mode of operating, the patient is never at ease during the whole time; the discharge of the fæces is often excruciating; even moving in bed is dreaded; and in the last case in which I performed the operation in that manner, the convulsive twitchings of the lower extremities which were induced, became so frequent and so violent, that I was uneasy through an apprehension of tetanus being the consequence. It seems to me probable that one reason of the difference between the effect of the wire and a common ligature may be, that however firmly the waxed ligature may be drawn round and tied on the base of the tumour, before a second knot can be tied to secure the first, the elasticity of the parts compressed opens the first knot a little, and of course the exclusion of blood and nervous influence is not so complete as when the wire is used, which can be fastened on the arm of the instrument at the time when it is drawn round the swelling as tight as possible. The pinch given by the wire is soon destructive, and any degree of restoration is rendered impossible.

It might be supposed if a thread were used, it could be cut off after a short time; but the swelling comes on so speedily, the parts retract so much within thea nus, and are so extremely tender to the touch, that it is difficult to find the noose: when found the operation of dividing it either with knife or scissors is productive of so much pain, that I have known some patients refuse to submit to it.

The removal of the wire occasions no pain.

It may be proper to mention, that when the tumour happens to be attached to the inside of the anus anteriorly, some difficulty in voiding urine is often complained of; but this symptom always, in my patients, has subsided immediately after the removal of the wire.

ART. III. *A History of a Contagious Fever which has prevailed throughout Italy during the greater part of the year* **1817.**

By JOHN BELL, M. D.

THE sources from which the following account is **drawn** are numerous and diversified, and may perhaps enable **me** to give a more ample and impartial history of the **disease** than would be found in the publication of any one of **the** physicians of the country, however enlarged his opportunities of observation or confident he might be of the correctness of his principles.

The most usual name given to the disease, which, during the greater part of the year 1817, has pervaded all Italy and many parts of Germany, is *Typhus*-Petechialis. Some call it "Petechial Fever;" others, perhaps afraid of committing themselves, designate it under the title of "Contagious Fever," or simply "Febrile disease."

It is far from being regarded as of recent date, though it is not held to be indigenous. It appears from the researches of Rasori and Fracastori, to have been imported into Italy some centuries ago, and diffused by contagion, like small- [illegible] and lues venerea. The hospitals furnish every year, spor[illegible] cases of it, and its general diffusion last winter and s[illegible] attributed to an epidemic constitution of the atmospher[illegible] ing a predisposition, operated on by scanty and bad diet, h[illegible] and the confinement of numbers together in an impure air. The poor being deprived of their usual nourishment in the country, advanced towards the cities, and not having the necessary shelter afforded them, flocked together to protect themselves from the inclemency of the weather.

Such is, according to Palloni, a physician of eminence in Leghorn, the causes inducing the disease in Tuscany; and he adds, that when so developed, it would attack by contagion others, who might not even be in the same situation as those just described.

Valentini of Rome, in his memoir on the disease as it appeared there, attributes its origin to causes nearly similar. The first victims to it, were the poor wretches confined in the prisons and hospitals.

The years 1630 and 1766, are referred to by the first mentioned gentleman, as periods when all Italy was affected with typhus, in consequence of a great scarcity of food which preceded its appearance. Camp typhus, which is a variety of this kind of fever, has for many years following the vicissitudes of war, been laying waste various countries in Europe.

Tommasini, Professor of the Institutes and Practice at Bologna, and one of the most celebrated physicians of the day, in treating this subject says, "The petechial or typhus fever, call it which you will, with which Italy is now affected, is not a disease different from that commonly observed in a few individuals every year; but the grades of predisposition in bodies to take it on, is different, and it is this greater degree of predisposition, that actually renders this fever epidemic. It is never developed and never generated without contagion; but this appears in many, because it finds many bodies susceptible of retaining its action, and it is this general susceptibility that renders dangerous and fatal during this year, that communication which in former ones was not to be feared. The variolous poison offers an example of this position." Again he remarks, "*An epidemic constitution never exists without contagion.*" The only writer I have met with who denies the contagious nature of the disease, is Buccellati of Pavia. He thinks that "if we carefully examine the persons seized with petechial fever, and others of a similar nature, we shall find a scorbutic diathesis prevail in all; that is, they have in their organization a peculiar tendency to organic dissolution, and will always discover these fevers the exclusive effects of worms or *vermination*," ("*verminazione.*")

That it is not inevitably contagious, I know myself from experience. When at Rome, in the large hospital of St. Esprit, with perhaps more curiosity than prudence, I visited and examined a man in the very height of the disease, not simply feeling his pulse, but running my hand over the surface of his body.

The most common diseases preceding and accompanying the one under consideration, were remittents of a bilious and gastric nature. Valentini says, that since the beginning of last October, gastric and nervous fevers, convulsive affections and apoplexy, with every species of madness, have prevailed. He adds that he has never seen so many in a furious and delirious state, as when severe cold follows south-easterly winds and rain, a state of weather at that time prevailing. At the commencement of the present year, these diseases retained the same nature, but all accompanied with gastric uneasiness. Peripneumony and acute pleurisy were rare, whilst bilious pleurisy was very common.

Some, among whom is Palloni, consider with Hildebrand and others, the petechial typhus as a distinct eruptive disease, arising from a contagion *sui generis*, like small pox or measles; that it is the effect of miasma introduced into the system, and which assimilating to itself all the animal fluids, is brought to the surface of the body, more particularly to the cutaneous capillary system: and also, that this typhoid is distinct from the common petechial or miliary eruption or from scarlatina. The first is a primary and essential exanthemata, differing from petechiæ, which vary in appearance, and are always symptomatic. He further thinks the former poison may be destroyed by oxygen; and hence urges the necessity of attention in preserving a pure air, by ventillation, fumigation, &c.

Opposed to this opinion are professors Franceschi of Lucca and Valentini, who view the eruption as by no means an essential feature of the disease. In proof of this, it has been remarked, that the eruption is sometimes absent, sometimes papillary, &c.

The premonitory symptoms of the disease are, pain in the head, muscles, and bones, especially in the loins and inferior extremities—want of appetite, little sleep, universal prostration of both physical and intellectual power.

After the disease is completely formed, the patient suffers from an increased pain in the head, greatest over the eyebrows, watchfulness, involuntary shedding of tears, stillicidium from the nose, heat and pain of the fauces, violent thirst—the tongue moist, covered with a whitish yellow crust and red at its edges—depraved taste, a sense of bitterness in the mouth, and weakness of stomach, with a desire to vomit—eyes inflamed and sparkling, cough and a troublesome sensation of tightness at the scrobiculus cordis, dry skin, a sense of burning heat alternating with cold fits all over the surface of the body—pulse weak and quick, although sometimes full and vibrating, bowels constipated. The fever often presents itself with the character of an intermittent, but soon takes that of a remittent or continued fever. Most commonly the patients are attacked, from beginning to end, with a violent acute fever, though cases are related where the fever was scarcely perceptible. The physiognomy is of a wretched and desponding cast.

Such was generally the first stage of the disease. In the second, that is from the fourth to the eleventh day, it became more aggravated: the pain in the head is augmented—some tension of the abdomen is felt, and, according to the particular viscera attacked, succeed either delirium or the symptoms of peripneumony and a sthenic affection of the liver and intestines, or of all the viscera at the same time. Delirium, the effect of cerebral inflammation, is often united to the inflammatory symptoms of the breast and those of the liver and intestinal tube—the tongue is observed to be red at its apex, dry, and loaded at its base—the looks of the patient are languid and vibrating—the tongue and hands tremble—there is a pain or *tinnitus aurium*—the pulse is weaker and more frequent—the excretions are small at this time; petechial eruptions, muttering, and delirium at night next succeed.

As respects the petechiæ, Palloni remarks, that from the third to the seventh day, there appears on the skin an eruption of red spots, pointed, irregular, and slightly scabrous and elevated (constituting the true typhoid eruption) among which may sometimes be seen petechial spots. The eruptions on the neck and shoulders, where it commences, soon extends over the whole body. Though the fever does not cease with the coming out of the eruption, certainly the internal agitation diminishes, and the catarrhal symptoms cease.

In opposition to this account is that of Franceschi, who has only observed the eruption on the inside of the arms and thighs, on the breast and lateral parts of the neck, and never on the face. He says, petechiæ do not terminate as other exanthematous or acute diseases of the skin, for no disquamation succeeds their disappearance, nor do any traces of their presence remain. The eruption is neither cutical nor symptomatic, as is shown from the uniform shape of the spots, and from their affording no relief, or causing any detriment when they appear.

When the morbid symptoms above enumerated become milder, with a purple, distinct, and well marked eruption, the fever goes on gradually declining from the seventh to the eleventh or fourteenth day. Dulness of hearing, sometimes a slight epistaxis, profuse sweats, cloudy and turbid urine, indicate a happy progress of the disease to its termination, which occurs from the fourteenth to the twentieth day. There is always, however, a great disposition to relapse, and we can only calculate after another week from the cessation of the fever as the beginning of true convalescence. But, on the other hand, if, on the approach of the fourteenth day, the disorder increases, then supervene nervous symptoms, *subsultus tendinum*, furious delirium, or it degenerates into lethargy. The tongue appears dry and covered with a blackish mucus: the throat is affected, and the fauces become aphthous—the teeth and gums are covered with a black tenacious mucus—there is anxiety about the precordia, spitting of blood, falling and irregularity of the pulse. The petechiæ alternately disappear and return, grow livid, and sometimes run into each other, forming blotches or *vibices*, which soon become gangrenous—the stomach is tumid—all the evacuations are scanty or suppressed, or involuntary discharges take place—the face is of a livid red, and sometimes the surface of the body is of a leaden colour —general immobility prevails—gangrene appears in some part—stertorous breathing—death.

A pulmonary attack at an advanced stage of the disease is a very dangerous symptom.

The fever that appeared among the beggars and criminals in the prisons at Rome assumed a somewhat different type. Thus the most common symptoms in it were, pain at the umbilicus, bilious vomiting, frequent dejections or gripings, continued thirst, watchfulness, delirium, tongue dry and covered with a black crust, convulsions, unequal pulse, burning heat, the face either very red or pale, and the countenance stern or sad. Many, on the appearance of the fever, were covered with petechiæ, purple or bluish spots, tending to black, and oppressed with great drowsiness and watchfulness, raving delirium, spasm, and not unfrequently lumbrici were discharged by the mouth.

Iscuria was of common occurrence in the disease; suppuration of the parotid glands, or mortification of the parts on which the patient rested was less frequent.

In laying down the plan of cure, all the writers already mentioned premise their observations on the subject by remarking that the physician can do little else than follow Nature, who is to perform most of the cure. The Roman professor, after expressing his entire conviction in the belief of the *Vis Medicatrix Naturæ*, says that the treatment of popular diseases consists solely in observing their course, and not administering any remedy until pointed out by Nature. Here then, in this country, Nature is deified. With us she is, after the precepts of some, to be treated as a mischievous intruder, to be turned out of the sick man's room. Perhaps, like many reformers, we may have gone too far, and it might be deemed most prudent to respect what we cannot in conscience worship.

There is considerable harmony of sentiment in relation to the treatment of the prevailing disease, and where differences arise, the discussion is conducted with a caution and moderation worthy of imitation.

It is generally agreed that there is in the commencement a morbidly increased action of the system. Palloni thinks that a phlogosis of the membranes of the brain constitutes the principle of the disease, from the morbid action which the contagion exerts in preference on this part. Hence the headache, disorder of the mental faculties, and the general orgasm, so that he concludes the debility observed at first, to be more apparent than real. The opinion of professor Franceschi, is not very dissimilar, when he says, "and as it appears that the petechia has a predilection for the brain over every other viscera, so we often see prostration at the commencement of the disease from the phlogistic pressure which it exercises over the sensorium: hence that apparent nervous convulsion which, by a common error of judgment, is often regarded as the effect of weakness, that is to say, as an asthenia, a product of the disease." Again, "According to the idea espoused by us, of petechiæ being often the result of a contagion which modifies sthenically the moving fibre in such a manner, that a voluntary depression of excitement cannot ensue as in other ordinary diseases of simple increased action, two consequences necessarily result: the first, that the grade of irritative inflammation developed, being different, by reason of the difference of susceptibility in individuals to receive the action of contagion, so ought the method of cure to vary in the different subjects attacked. Secondly, that this disease, as the result of contagion, may be in some measure restrained by adequate means, though it is not in our power to arrest its course."

The first remedy, and one of the most importance, in our disease, is an emetic of ipecacuanha, or if the coats of the stomach be lined with a tenacious mucus, tartar emetic is to be given. This, which may be often repeated, serves not only to cleanse the stomach of its impurities, but, by the debilitating and *counterstimulant** effects of nausea and vomiting, is often sufficient to lessen the inflammatory orgasm of the system.

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* This has become a favourite word in Italy, as it has given the title to a new medical theory lately broached here under the auspices of Rasori, Tommasini, and others. It is called the "*New Italian Medical Doctrine.*"

It induces also a mild and equable perspiration over the whole surface of the body, relieves the pain of the head, renders the pulse fuller and more regular, and diminishes fever. Next, the milder purgatives are prescribed, as manna, cream of tartar, &c. But where the presence of worms is evinced, then castor oil, in doses of two or three ounces, or calomel, in the proportion of twenty-four grains in the day, united with four or six grains of gamboge or jalap, and administered at intervals, are deemed the most efficacious remedies. The use of calomel in the practice here is restricted mainly to its vermifuge operation.

General bleeding is almost universally prohibited. But in cases of flushed face, sparkling and inflamed eyes, slight epistaxis, hot and dry skin, with a pulse hard and vibrating, local blood-letting is recommended, and is performed by applying leeches to the temples or scarifying cups to the neck and shoulders. In circumstances still more urgent and threatening, general venesection, even to taking away blood from the jugular vein, may be admissible. The symptoms menacing the breast, liver, and other viscera, for the same reason, require bleeding proportionate to the strength of the patient, or intensity of the attact. Thus says Franceschi, who adds, however, that in ordinary cases sanguineous evacuation may be usefully dispensed with.

The application of cold to the head, by means of bladders filled with snow or ice, and sponging the surface of the body with cold water and vinegar, have, according to the testimony of some, converted the most violent delirium into a tranquil sleep—while others think this course, if not injurious, of equivocal benefit, and apt to drive in the eruption.

Blisters are recommended, where there exists a medium state of excitement, where the great cavities are threatened, and in cases of torpor of the parts, or where a wandering pain may be fixed externally. They are contra-indicated in a state of irritation, in a sanguine or nervous temperament, and where there is a tendency to dissolution of the solids.

When these remedies are found unavailing, and the second stage, or that of real debility succeeds, stimulants and cordials are to be given, such as the decoction of serpentaria with bark, extract of bark with camphor, colombo root, generous wine, musk with anodyne liquor, *calamus aromaticus* in the proportion of two or three drachms, to make an infusion when strained of four ounces, dose a table-spoonful, or calamus with bark. When delirium has supervened, the pulse being soft and full, laudanum has been administered with success, and sinapisms to the soles of the feet have been useful for the same purpose.

The infusion of polygala senega united with acetate of ammonia, often favours a free typhoid eruption, prevents the dryness of the skin and is serviceable in the catarrhal affection, so often accompanying the disease. Prompt measures have brought back the eruption when it has receded, and to this end, sinapisms and blisters, and the internal administration of a few ounces of camphorated vinegar, given at short intervals and the warm bath, have been found useful. The use of elixir of vitriol, with a decoction of simarouba and nitric acid, diluted in common water to a state of pleasant acidity, has succeeded in checking *melena*, profuse sweats and diarrhœa. Volatile liniment, warm fomentations, camphorated clysters with assafœtida and musk, have checked mental wanderings and nervous affections, and have even snatched as it were the poor sufferers from the jaws of death.

To prove how much nature is capable of performing, and to what extent we should rely on her exertions, Valentini says that in a great number of cases of this fever, he has administered nothing but copious draughts either of pure water, or of lemonade, or nitrous emulsion. He cites as a proof of the efficacy of this plan, the case of a soldier in the Pontifical service, thirty-three years of age, and of a robust habit, and who was seized with the gastric-nervous fever. This man obstinately refused all medicines, except an emetic which had been given at the commencement of the disease;

yet, by drinking simple lemonade and abundance of the purest water, after having suffered the attack of mortal symptoms, on the seventeenth day of the disease he fell into a copious sweat and recovered. I was persuaded, continues he, of the inefficacy of medicines in the greater number of cases, and willingly followed the wishes of the patient. He supports his opinion by referring to Hippocrates and others of the ancient, as well as modern writers; and concludes by a quotation from Celsus, in which that author says, "*multi magni morbi curantur abstinentia et quiete.*"

The drinks best adapted to the disease are the subacid and emeticized, as cream of tartar, in the proportion of one ounce to three pints of water, sweetened with sugar, to which may be added one or two grains of tartar emetic—lemonade with the same—toast and water, &c. Fruits, moderately acid, light broths, and panada, may answer for nourishment. More substantial diet, besides the repugnance of the patient to it, would not be proper in the first stage of the disease.

The convalescence from the disease is generally tedious, and requires great attention. Stimulants, external and internal, natural and artificial, must be measured out with the greatest caution. Simple food, a little generous wine, gradual exposure to the open air, long rest, light and agreeable occupation; a simple infusion of bark and valerian, and clysters to keep the bowels open, constitute the diatetic plan, during the convalescence of petechial typhus. Nor does the disease secure the convalescent from a fresh attack of the action of the contagion. Some assistants in the hospitals have been twice attacked by typhus, at no great interval between the periods, with a copious eruption and all the other characteristic symptoms, and have fallen victims to this second attack. Females are more liable to catch the disease than males, and old and infirm men more than the adult and robust.

The prophylactic plan consists in tranquillity of mind, nutritive diet, cleanliness and ventilation of the rooms, avoiding excess of every kind, and shunning with the greatest caution contact with the diseased or with their clothes. Where it is required from professional duty or otherwise that connexion must take place, the person thus exposed, on leaving the sick room, should wash his hands and face well, and change his clothes.

Those who are most averse to the abstraction of blood when the disease has actually appeared, are nevertheless disposed to think favourably of it as a preventive, in persons of a full habit and sanguine temperament: in this they are supported by the authority of Hoffman. It has been said that those labouring under phthisis, or in whom an artificial drain is kept up by issues, &c. are safer than others from the action of the contagion.

The various means of disinfection by washing, fumigation, &c. as recommended by Carmichael Smyth, Guyton Morveau, &c. are so well known, that it is unnecessary to repeat them here.

Thus concludes the treatment of a disease which, says Franceschi, " actually pervades a great part of Italy, and which at one time clothed in the ordinary characters of petechial typhus, at another of peripneumony or of an inflammatory affection of the lungs, then of an attack on the liver or intestines, requires at least prudence either in debilitating too much, or what is still worse, provoking by exciting remedies the increased action of the disease, under the false idea of *spurious inflammation* or inflammation of weakness, a name unphilosophical and entirely banished from sound medical reasoning, and to which we may apply what the great Sydenham says of the word *malignant*, that it has committed greater ravages than famine, plague or the sword. Inflammation may, it is true, differ in grade and intensity, but nature must be always identical. If inflammation arise in a weak subject in some one of the viscera, debility supervening, only proves evidently that although the system languishes, the inflammation, sustained by some local cause, pursues in an uniform manner its course, independent of the opposite state of the system. Now if in such a case, we cannot debilitate, much less ought we to have recourse to heating remedies, the use of which experience shows to be dangerous and hurtful."

The appearances after death on dissection, were as follows: in the cavity of the thorax there was sometimes observed considerable serous effusion. The lungs were often hepatized either throughout their whole substance, or more commonly in one lobe rather than another, either the right or left. Purulent spots have been observed, and sometimes a suppuration and almost entire destruction of one or both lobes. The abdomen has presented morbid appearances, varying according to the different viscera affected. The liver has been found sometimes adhering to the diaphragm and peritoneum in various places, and covered on almost its entire convex surface, with a thick stratum of condensed albumen: its substance is at one time natural, at another altered in various ways, and its volume increased. The stomach and intestines were more or less inflamed in particular spots and twisted and contracted, sometimes distended with a fetid gas, or containing altered bile, and lumbrici and ascarides—and the omentum almost destroyed by mortification. In some instances the peritoneal cavity was found in a healthy state: in others with a more or less sensible alteration, and with a serous extravasation.

In the cavity of the cranium was observed great engorgement of the membranes of the brain, most observable in that part covering one or other hemisphere. Between the dura and pia mater, there existed in some subjects an extravasation of fluid blood, in others it was grumous: at other times these two membranes were found adhering to each other. The longitudinal and other sinuses were more or less full of dark blood. In the ventricles there was often found an effusion of serum or bloody lymph. The substance of the brain has shown in some cases a certain softness: most frequently it was of a healthy appearance. The external surface of the body did not in many undergo any particular alteration; in others, from the malignity of the disease with which they were attacked, it was covered with bloody or gangrenous stripes or spots, especially on the back, breast and extremities. The muscles were very flaccid, and the passage into putrefaction rapid.

In the two estimates which I have seen of the mortality of the disease, the deaths in one, made out at Leghorn, are calculated at six, in another at Florence, three out of two hundred: moderate, indeed, when compared with the universality of the disease.

On the appearance of disorders of this kind, all the Italian governments adopt the most rigid measures for separating the diseased from the healthy, and establish hospitals for the express purpose of recovering the former.

The typhus-petechialis was at its greatest height in March, April, and May, and though it continued much longer it lost its violence.

Florence, Oct. 10*th*, 1817.

ART. IV. *Cursory Remarks in relation to the Objects of the Materia Medica, and Pharmacy, &c. By* JOHN REDMAN COXE, M.D.

IT has been remarked, and with some propriety, that a great book is a great evil. The observation may more justly be applied to an extensive Materia Medica, and to a diversified Pharmacy.

I believe it will be very generally admitted, that no physician, however extensive his practice, does actually employ more than fifty different and distinct articles; probably not even a third part of this number. If such is the fact, it may admit of a question, why we are so anxious to extend the boundaries of this branch of our profession; and whether every thing that can be anticipated from a greater variety of less active articles, may not safely be expected from a very few of the most efficient?

Among the evils of an extensive pharmacopœia, we may, I think admit, that it renders us incapable of duly estimating the real value and medicinal properties of each remedy in particular. Experience, the only touchstone of the merit of each, is too extended, to admit of just and perfect observation; our views become embarrassed and uncertain; and our deductions are not always sufficiently accurate, to preclude the contradiction arising from other authorities. Hence we might safely affirm, that we are absolutely unacquainted with the full and complete powers of any article we employ! There is scarcely one, which can be said to correspond fully, with the exaggerated accounts of those who first introduced it into notice. If we need an example in proof, we have only to look at the different opinions of the efficacy of digitalis, and of hundreds of other remedies which at present are employed; or which, having once stood high in the estimation of physicians, have sunk into the oblivion they so well deserved. Will the present admirers of guaiacum, now maintain its powers, as being equal to those which were first attributed to it, when it was sold for its weight in gold! Where are now the boasted specifics for canine madness? Celebrated in their day, they are superseded by others of equal inefficacy; and yet, all have had their advocates, pleading from experience in their behalf.

If such is the deficiency of remedies which formerly were so highly rated, and which for years, maintained so elevated a rank in the materia medica; can it admit of a doubt, that it is incumbent on us, to be careful of introducing new and unknown ones, before their merits are *fully* and *clearly* established? Shall a remedy of doubtful, or of trifling powers, be added to the list, because it happens to be a product of our own country; and ought it to supersede one of foreign origin, which is known to be superior in virtue?

It may be asked, by what authority do most of the new articles introduced of late years into our Pharmacopœias, &c. come recommended. Unquestionably, in many instances, from that of practitioners or others, who frequently are ignorant of the nature and properties of even those of long-established use; and who are probably often incapable of discriminating the very articles which they employ in practice. A few imperfect experiments, founded on the inadequate observations of some local inquirer; experiments devoid of any specific object, and often injudiciously made; too frequently excite to a desire of appearing in print, and of recommending a remedy as possessed of the highest virtues, which subsequent experience in other hands, shows to have been incautiously attributed to it; or the deductions to have been drawn from incorrect and inadequate observations.

If we look into the materia medica from the most distant period to which the records of medicine extend, how very few are the articles which have reached the present times, amid the fluctuating opinions of medical practice! It is believed, that scarcely one hundred, of perhaps thousands, which have been noticed, are now to be found on our lists. A few of real importance, have maintained their ground; the rest have passed away, and are slumbering in oblivion. Some have, it is true, again found admirers, but they also have occasionally a second time sunk into neglect. If we even look at those which have passed the ordeal of time, we shall perceive but few, whose asserted virtues have not been more or less assailed: so that, judging of them from such adverse opinions, we might almost say with Pilate, " What is truth!" Authorities so discordant, tend to show, how uncertain is the basis on which we place our confidence in medicine. It is almost as insecure as that which placed the earth upon an elephant, and this upon a tortoise. Here the foundation failed; and thus too often shall we find our best remedies fail in their effect: all may at times be useful, at others, prejudicial; their effects are relative, and dependent on the living system. The food which to-day is eaten with impunity, will, to-morrow, from changes in the system, induce dyspepsia; and habit will enable us even to swallow poisons without any serious inconvenience.

The above observations lead me to remark, that if the truly efficient, but small number of articles of the armoury of medicine, are incapable of doing all we wish; it is scarcely to be expected that an extension of their number will add to our means of cure. It is not number, but quality we want, for the few indications which our profession points out to us!—and if this is true, it is obviously of secondary consideration, from whence we derive those articles which are entitled to our confidence, provided they are the best which nature offers. We want the most efficacious that can be procured, without regard to their locality or price. If such are to be procured within the limits of our own country, let us cheerfully admit them into our list; but if better are procurable from foreign sources, it is our duty to prefer them. What argument can prove, that because the dogwood abounds amongst us, we must prefer it to the bark! why should the spiræa, usurp the place of ipecacuana! what signifies the inferiority of price, at which these and other articles of domestic growth can be procured, if in medicinal virtue they are inferior to those of foreign origin! There is but one reason that can be urged for their employment that I can perceive, viz. an impossibility of obtaining the others; it is the only excuse for a *quid pro quo;* and is of the same value with that which would urge us to feed on potatoes, if wheaten flour could not be procured.

If we can domesticate any of the useful remedies of foreign origin, reason will justify, and success will probably follow the attempt. The poppy, rhubarb, and many more, are examples of this description; and by proper attention, our own soil will abundantly yield the important remedies derived from them. For many of our most valuable articles, we have as yet, however, found no substitute amongst ourselves. The mercurial and antimonial preparations must still reach us from a foreign source; or at least, the raw materials from which they are derived.

Whilst I am thus opposed to the rage of introducing, at all events, new remedies within the limits of the materia medica, merely because they originate amongst us; it must not be supposed I am inimical to the most free and constant inquiries into the medicinal powers of our own plants and animals! Such inquiries are in the highest degree praiseworthy; and may, it is hoped, lead to the discovery of articles equal in efficacy to those derived from abroad; but if such inquiries should demonstrate that we are already possessed of similar articles of superior worth, it is then but labour misapplied to attempt to introduce them; and can only, by increasing the number of inefficient drugs, render confusion worse confounded.

It is time now, in investigating the medicinal powers of any article, *whether foreign or domestic*, to recur to fact alone, founded on experience; and that *confirmed* by the testimony of others. By such means, we may hope to establish a small but efficient list, which may be safely recommended to the confidence of practitioners.

Our new and national Pharmacopœia, if properly conducted, may happily diminish the disgusting catalogue of drugs without weakening our means of cure. It may in this particular, boldly set an example to the world, as our country has in politics, which sooner or later will be followed. The lopping off the governmental trappings which our revolution accomplished, has given us a corresponding augmentation of strength and national utility. The idle and unmeaning paraphernalia of monarchical governments, are like the useless appendages of our materia medica; they make a show, which adds greatly to the expense, but not an iota to its real utility. Let us not then spare them! let our materia medica, like our government, be simple, but energetic; and our posterity will have reason to thank us for the trouble we have saved them. If in fact, we really desire to strengthen our means of cure, it must be, by becoming more fully acquainted with the real merits of those remedies we may think proper to retain. We must without reserve, discard whatever is inefficient; nor, even of active remedies, is it requisite to retain so many of the same nature! If one or two, will readily supply every indication for which they are prescribed, why should we unnecessarily augment the number? Whoever peruses with the slightest attention, the numerous Pharmacopœias or dispensatories of the last and present century, may readily perceive to what an epitome they might be easily reduced. How many of the raw materials are superfluous; how many compounds might be spared;

and of the compounds retained, how many might be simplified! The indications to be answered in the practice of physic are few in number; not less so are the means, by which such indications may be fulfilled. Let us briefly enumerate some of the principal articles, and see, if with these we may not safely rest contented.

Amongst the *cathartics*, can we possibly be deficient, if we limit the number to about a dozen; viz. jalap, rhubarb, senna, manna, castor-oil, salts, cream of tartar, sulphur, magnesia, aloes, and above all calomel?

For *emetics*, can we possibly require more than ipecacuana and emetic tartar; together with two or three others of inferior powers?

Of *diaphoretics* and *diuretics*, some of the before mentioned will be found useful; squills and seneka, perhaps digitalis, a few of the neutral salts, and more especially, aqueous dilution.

As a *sialagogue*, can we expect any thing superior to mercury?

Our *expectorants* are chiefly of an emetic nature, and may embrace those above mentioned.

Our *antilithics*, are either some of the mineral acids, or alkalies: and our *emmenagogues*, (a most uncertain class,) are occasionally of the evacuant or of the tonic kind.

Amongst the *tonics*, independently of diet, we chiefly depend on bark, columbo, quassia, some few of the metallic preparations, as of iron, copper, zinc, and arsenic; and amongst the *stimulants*, what can we require beyond wine or alcohol, opium and its preparations, some few of the essential oils, ether, oil of amber, and a small number of inferior powers?

Our external applications are derived from cantharides, one or two caustics, and a few more; together with some articles for the formation of ointments. And if, with all these, scarcely exceeding half an hundred, we cannot accomplish our intentions, it is much to be feared, success would not follow from a greater augmentation.

How few, indeed, of those I have mentioned, are there, which are actually employed in the practice of any individual physician! His real pharmacopœia is usually circumscribed within much smaller limits; and he practically finds, that a rational and judicious selection of a very few remedies, will subserve the interests of his patients better, than a luxuriant variety of random articles.

If any radical reform is to be expected in the materia medica, it must arise from the conjoined attention of physicians themselves: aware of the facility and of the universality of deterioration in the articles they employ, either by the wholesale druggist or by the apothecary; they will readily perceive, that one of the chief means of obviating so culpable a fraud must be, that of so far diminishing the number of articles, as to enable the practitioner to become better acquainted, and even familiar, with the few which may be reserved for use. It is the interest of the apothecary to have a large materia medica: it is that of the practitioner to have a small one. As the business of the apothecary is too often now conducted, it is his interest to have the number of his articles increased, attending to the price rather than to the quality. This deterioration, however, of our remedies, is not wholly to be ascribed to the apothecary. It is, perhaps, primarily dependent on the false economy of physicians themselves, who too often purchase their medicines at the *cheapest* store, even although, if they give the subject a momentary consideration, they will be satisfied that the price is really incompatible with the perfection of the article. How can a medicine which, as imported, costs, perhaps, a dollar per pound, be sold at more than twenty-five per cent. loss? Lately we have seen, that opium has been sold in our market at about three dollars and a half per pound, and yet good opium, I have been assured, would command six or seven. Under the name of Persian opium, an article has been largely vended, which appears to be sophisticated with tobacco juice and other foreign ingredients. The false economy above spoken of, leads, I am persuaded, to this and other frauds, which would not otherwise be thought of. It leads, necessarily, sooner or later, to the sophistication of articles of primary importance; and the upright and independent practitioner becomes equally tributary to such dishonest proceedings as the penurious pretender in medicine. I have seen at the sales of drugs, seroons of bark sold at sixpence per pound, which I think no honest man would choose to purchase; mildewed from dampness, and scarcely fit for the dunghill; yet such has been pulverized and sold in our city, mixed up, I believe, with a small amount of superior value, and at prices equal to that which the very best commanded. All the articles of the materia medica are prepared for sale according to the prices at which they are demanded. Many of them, effete from age, from damp, or other causes, are palmed upon us; and the lives of patients are jeopardized from the most ignoble motive. Compounded of such materials, our prescriptions, devoid of energy, are uselessly administered; and their place might be equally or better supplied with powder of post or bread pills.

It is time to adopt measures by which, as in Europe, an inspection of medicines may tend to substantiate their perfection. The apothecary, anxious for the honour of his profession, would cheerfully permit it. The unworthy member of it would soon be known, and the character of his drugs would deprive him of public confidence. It is time to give more importance to the apothecary in his own estimation, and to exalt him in that of the public, by granting, under the sanction of our universities, such honorary distinctions as may be deemed proper, to all those who, by a regular apprenticeship and due examination, shall be found deserving of encouragement. Such distinctions, whilst they would excite to a laudable emulation, would, at the same time, make every generous mind esteem his reputation beyond the sordid consideration of gain alone.

It is, in fact, a great misfortune for our students generally, that the good old custom of preparing our own medicines has become so completely abolished. Few of them will enter into and pursue the drudgery of a drug store;

and hence they have scarcely a chance of becoming acquainted with even the external appearance of the articles they are hereafter to employ. They know by rote the doses they are to be employed in, and the indications with which they are to be prescribed; but present them to their view, and so far from knowing whether they are perfect in their kind, they cannot always tell if they are really the articles they wish to employ. This ignorance was not known in former times. The student, under the direction of his preceptor, became accurately acquainted with the means of cure; he could prepare them for use in an appropriate manner; he could readily prepare the pills, the powders, and mixtures of his master's practice; he could not only spread, but also dress a blister, when required; and accomplish all those other duties of his profession which are often regarded as disgraceful, and which yet he must, in practice, be sometimes called upon to perform. Nothing, however menial it may be considered, is really disgraceful in the practice of physic. The whole business of the profession is, in truth, completely opposed to comfort, to decency, and to decorum. It is the abandonment of self for the convenience and benefit of others. No one, in the practice of surgery, considers himself diminished in importance or self-respect, when performing the most disgusting operation, even on the lowest member of society; yet the less offensive manipulations of pharmacy are regarded as of little importance, and are now committed to the hands of the most inexperienced shop-boy.

The most illustrious members of our profession, in former times, did not esteem this branch as unimportant. In the times of Hippocrates, of Galen, &c. the apothecary and the physician went hand in hand. Nay, many of those who may peruse these lines are aware, that in the shops of their preceptors, they themselves derived advantage from the same conjunction. Why it has of late years so completely been abolished, I confess I have yet to learn. Our practice is not more laborious than theirs, and I think it is a duty incumbent on us to resume the custom. It is a practical branch of the profession, which can in no other way be so fully acquired by the student; and it is obviously from this neglect of a good custom, that so many ignorant apothecaries have sprung up amongst us. Fifty years ago there were scarcely two in the city of Philadelphia, and they, chiefly wholesale druggists rather than apothecaries, importing the raw material for the practitioner. Now, I presume, we have an hundred, of whom scarcely one in ten can be considered as competent. Many have entered into the business without any preliminary qualification; and, except by name, scarcely knew an article of their *trade*, until it was purchased for the shop they had thus newly established.

From these, and other causes, has arisen the want of uniformity of the same medicines in the different shops of our city. Bound down to no particular Pharmacopœia, each one prepares them according to his own conceptions; often from some private recipe, differing entirely from that of the colleges. The prescriptions of the physician are, of course, altogether uncertain, and stronger or weaker according to circumstances.

Whether this fault will be rectified, by the introduction of our proposed National Pharmacopœia, remains to be proved. It is to be hoped our physicians will endeavour to give it full effect, by sending their prescriptions to no one, who will not strictly conform to the directions and recipes therein contained. It is not so much the strength, as the perfect uniformity of our remedies, by which we may confidently trust that we are invariably prescribing the same, no matter from what shop the prescription may be derived.

I am, however, not the less persuaded that, as it regards the medical education of those who place themselves under our direction, it is our duty to give them that practical instruction in pharmacy, in which they are, for the most part, deficient. They require it, and they deserve it at our hands, fully as much so, as that of any other branch of medicine which, as preceptors, we undertake to teach.

Before I conclude these hasty remarks allow me to ask the reason of the great delay in bringing forward the Pharmacopœia, the basis of which was so long since agreed on. The medical public are looking with anxious expectation to its appearance; and, aware of the capability of the characters to whose care it was committed, it cannot but excite astonishment that a work so important should be so long withheld.

September 26th, 1820:

Art. V. *Remarks on Bronchocele or Goitre.*

By W. Gibson, M. D.

THE terms Bronchocele, tumidum Guttur, Hernia bronchialis, Gongrona, Hernia gutturis, and others of similar import, are employed to denote a morbid enlargement of the thyroid gland. The word Goter or Goitre, was invented by the Swiss, and is probably a corruption of the Latin phrase Guttur. In England the disease is known in popular language, under the name of Derbyshire neck, or monstrous craw.

Bronchocele has prevailed, in certain countries, from time immemorial. It is noticed by some of the ancient poets, and by many of the early writers on medicine. It is met with oftener in mountainous than level countries, and is frequently endemial and hereditary. According to Coxe, the disease is common in the neighbourhood of Berne, Friburg, Lucerne, Aigle, Bex, Dresden, in the vallies of Piedmont and Savoy, in most parts of the Vallais in the Valteline, &c.* In the village of La Batia, Dr. Reeve saw many cretins and goiterous persons, who all lived in adjoining houses.† The village of Villeneuve d'Aoste, which is surrounded by very high mountains, contains an immense number of persons who labour under goitres of enormous magnitude.[*] My friend Dr. Howard of Baltimore, during his late rambles in Switzerland, first met with goiterous persons and cretins near Sion. The number of each continued to increase as he approached Martigny and St. Maurice, at which places they are exceedingly numerous. As he descended the Rhone their numbers decreased. In the year 1800, the villages of St. Jean, St. Michel, St. Maurice, and the vicinity of Aiguebelle, according to Foderé, contained a greater proportion of cretins and persons labouring under goitre than any other part of Switzerland. Dr. Howard was informed that both cretinism and goitre had diminished within the last few years, in consequence of the richer inhabitants sending their children, until their tenth or twelfth year, to the mountains, where their wives also remained during pregnancy, and for some time after parturition. In the mountainous parts of Spain and Germany goitre prevails to a considerable extent. In France it is chiefly met with in the districts of Cevennes, Soissonais, Vosges, Rouergue, Doubs, and Ardeches. In England it is very common in the mountainous parts of Derbyshire, in Buckinghamshire, Surry, and in the county of Norfolk. Occasionally it is seen in Nottinghamshire.[†] Sir George Staunton says, that goitres are very common in those parts of Chinese Tartary which resemble the mountains and vallies of Savoy and Switzerland.[‡] "In Bengal," says Turner, "this unsightly tumour is known by the name of gheig and aubi; and in Boutan is called bà or ke ba, the neck swelling, and forms itself immediately below the chin, extending from ear to ear, and sometimes growing to such an enormous size, as to hang from the throat down upon the breast. It is particularly observable among the inhabitants of the hills of Boutan, immediately bordering upon Bengal, and in the tract of low country watered by the rivers that flow from thence to the south, beyond the space of a degree of latitude. The same malady 'prevails among the people inhabiting the Morung, Nipal, and Almora hills, which, joined to those of Boutan, run in continuation, and bound, to the northward, that extensive tract of low land embraced by the Ganges and the Berhampooter. The same disease is also more particularly met with in the low lands adjoining those hills. From the frontier of Assam, north latitude twenty-seven degrees, east longitude ninety-one degrees, it is to be traced through Bishee, Gooch, Bahar, Rungpore, Dinagepore, Purnea, Tirrooto, and Betiah, along the northern boundary of Oude, in Gooracpore, Barraitch, Pillibeat, and on the confines of Rohilcund to Hurdewar, situated in north latitude thirty degrees, east longitude seventy-eight degrees twenty-five minutes. It has the effect, or is rather accompanied with the effect arising from the same cause, of debilitating both the bodies and the minds of those who are affected with it."* Park, in giving an account of the diseases of the Mandingo negroes, states that goitres are very common in some parts of Bambarra.† Throughout the island of Sumatra, bronchocele is met with as an endemic disease, and is particularly frequent in those vallies which are surrounded by the highest mountains.‡ In some of the Spanish settlements of America goitres are so common, that the greater number of the inhabitants labour under the disease; and at the village of Jacaltenango, near Sacapula, it is said that no individual can be found without an enlargement of the thyroid gland.§ In Santa Fee, Guatemala, Nueva Gallicia, and Nicaragua, the complaint has long been known. It is common also among the Indians who inhabit the vallies of the Cordilieres. According to Humboldt and Bonpland, goitre is an endemic disease at New Grenada, and is so common at the small villages Hunda and Monpar, on the borders of the Magdelaine river, that it is difficult to find an individual who is exempt from it. It affects indiscriminately all classes of inhabitants except the blacks and those who lead a very laborious life. The ferrymen at Carthagena are not subject to it. Females are oftener affected than males. At the Isthmus of Darien many persons are horribly disfigured by enormous bronchoceles.* In various districts, and throughout whole tracts of country in North America, bronchocele prevails as an endemic. It is very frequent in many parts of Lower Canada, especially near the marshes between St. John's and Montreal. At Detroit, Lake Ontario, Oneida, Erie, Huron, and among the Tuscorora, Seneca, Oneida, and Brothertown Indians it is very common.† In many parts of the state of Vermont, especially Bennington and Chittenden, bronchocele is well known. It is also found at Camden, Sandgate, and Chester, in the same state. Sandgate, some years ago, contained one thousand and twenty inhabitants, and out of that number one fourth of the females were affected with the disease.‡ According to Dr. Trask, bronchocele is so common a disorder at Windsor in Vermont, that hardly any female is exempt from it.§ In the state of New-York goitre prevails principally in the neighbourhood of Old Fort Schuyler, the Oneida village, the German Flats, Fort Herkimer, Fort Dayton, Henderson town, Onondago valley, Canasaraga, Brothertown, the township of Manlius, and the whole of the military district.|| I am informed by Philip Church, Esq. who resides at Angelica in Alleghany county, state of New York, that goitre is a very frequent complaint in his neighbourhood and the surrounding country. In Pennsylvania where bronchocele is very common, it is found chiefly at Pittsburgh, on the waters of the Alleghany, Sandusky, Monongahela, French Creek, at Cannonsburgh, Brownsville and throughout the county of Somerset. In some parts of Virginia, especially at Morgantown and on the banks of Cheat river, it is by no means unfrequent. In certain situations on the

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* Coxe's Travels in Switzerland.

† Reeve's account of Cretinism, in the Edinburgh Medical and Surgical Journal, vol. 5. p. 33.

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[*] Saussure's Voyages dans Les Alpes.

[†] Clark's reports from the general hospital near Nottingham, in the Edinburgh Journal, vol. 4.

[‡] Staunton's Embassy to China.

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* Turner's Account of an Embassy to Tibet.

† Park's Travels in Africa.

‡ Marsden's History of Sumatra.

§ Barton's Memoir on Goitre.

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* Alibert's Nosologie Naturelle, p. 470.

† Barton's Memoir.

‡ Dorr's Facts concerning Goitre, New-York Medical Repository, vol. 10.

§ Mease's Observations on Goitre.

|| Barton.

western shore of Maryland, and in North and South Carolinas, the disease is occasionally met with. It is probable, indeed, that goitre may be found as an endemic disease, in almost all the mountainous and marshy districts throughout the United States. All writers on the complaint, agree that it generally prevails in vallies at the bottom of the highest mountains, which are particularly exposed to the influence of easterly and southerly winds. In those situations, moreover, where the temperature is mild and uniform—where the atmosphere is moist—in the neighbourhood of rivers, of falls or lakes, or of the sea,—where the soil is rich and the habitations surrounded by fruit trees, goitres are commonly found.

Every age and sex is liable to goitre, but females are oftener affected than males. In children it seldom occurs until after the eighth or tenth year, and old people are little subject to it. Three instances however are mentioned by Foderé, where it was found at birth, and another, in an infant fifty days after birth.* Dr. Sterndale, has also furnished an example, where a child in Derbyshire, was born with a goiterous tumour of considerable size.† Those females who are not subject to bronchocele before marriage, generally perceive its commencement during pregnancy.‡ Persons of relaxed constitutions, of white and delicate skins, and whose complexions are red mixed with a brownish tinge, are most predisposed to the disease. Children who are to become goiterous, have large blue, sprightly eyes, beautiful skins and fair hair. Their memory is very forward. When the disease appears, every thing is changed. As it advances the eyes become dull, the face acquires a white colour and unmeaning look, and the faculties are at a stand. When the goitre is very large, respiration becomes difficult, the pronunciation of consonants imperfect, and the body ceases to increase except about the head and shoulders. In a goiterous country, the children are born goiterous after two generations of the inter-marriage of goiterous parents. After the third marriage, the child becomes a cretin. A semi-cretin, weak and ricketty, married to a goiterous woman, has children born goiterous.[*] During the winter, a goiterous tumour is diminished in size, but it augments with the return of warm weather, and is larger during autumn, than at any other season. The disease is not confined to the human race; horses, horned cattle, calves, sheep, dogs and other inferior animals are subject to it.[†]

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* Traite du Goitre et du Cretinisme.

† London Medical Repository, vol. x. p. 200.

‡ Foderé, p. 62.

In the commencement of bronchocele, a small tumour may be perceived, either on one or both sides of the trachea and larynx. Sometimes the swelling occupies each lobe of the thyroid gland, together with its isthmus, so as to constitute a uniform tumour; at other times, there is a depression at the centre, following the course of the trachea, and marking the natural division of the lobes. Occasionally the enlarged lobes are studded over with a number of lobules. The swelling generally continues small and circumscribed for a considerable time, and often extends backwards, so as to render it difficult to ascertain by inspection or examination, whether goitre exists or not. For the most part it is soft to the touch, and possessed of so little sensibility, that it may be rudely handled, without producing much uneasiness. It is sometimes closely compressed by the muscles which cover it, and is then elastic and firm. Although the thyroid gland, both in its natural and enlarged condition, is not very susceptible of inflammation, yet when this state is once induced, it becomes exquisitely tender, and is accompanied with a difficulty of respiration and deglutition, which the most active antiphlogistic measures can hardly subdue. Almost all the goitres which have come under my notice in America, have commenced in one lobe of the gland—the other lobe in a short time being affected in a similar manner. Alibert says that he has found the right lobe oftener enlarged than the left.[‡] In the worst cases of goitre I have seen, the tumour has exceeded in si a large cocoa nut, and has become at particular times ver troublesome to the patient, by its weight and pressure upo the trachea and adjacent parts. In countries where the disease is endemic, it is not uncommon for the tumour to attain an enormous magnitude. Foderé relates instances of such tumours weighing seven or eight pounds.* A case is recorded by Alibert, of a man thirty-eight years of age, who had a goitre which extended below the middle of the chest and equalled in size a large pumpkin. "La poche énorme qui s'est formée au dessous de son menton, ressemble à celle de l'oisseau désigné communément sous le nom de *pelican*, et qui figure comme object de curiosite dans les cabinets des naturalistes."† The same author details the case of a female, upward of sixty years old, who had resided the greater part of her life near Chamouny at the foot of Mont Blanc, and who from her infancy, had laboured under a bronchocele, which was divided into innumerable lobes, which extended from ear to ear, blocked up the cavities of each, so as to destroy the hearing, and finally descended on the chest, lower than the mammæ, interrupting the breathing and swallowing to such a degree, as almost to produce suffocation, every time she attempted to take the least particle of nourishment, solid or fluid. But cases have been related by Mittlemayer and others, in which goiterous tumours have descended below the umbilicus, and even to the knees.‡ We have no reason to suspect these accounts exaggerated, when we remember the reports of Sir Robert Wilson, Larrey and others, respecting those prodigious tumours, common in Egypt and many warm climates, in consequence of the descent of the abdominal viscera, which in some instances have reached the ground. The cases of enormous hydroceles, also, recorded by Keate, and the voluminous cutaneous excrescences described by Mr. John Bell and lately by Dr. Roper of Charleston, leave no doubt on the subject.

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[*] Foderé, also Chapman's Notes on Allan's Lectures.

[†] Coxe's Travels, Barton's Memoir, Clark's Reports.

[‡] Nosologie Naturelle.

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* Traite du Goitre, &c. p. 467.

† Nosologie Naturelle, p. 468.

‡ Dissertatio de Strumis et Schrophulosis, 1723.

Notwithstanding the peculiarities of goitre, it is not easy, always to distinguish it from other diseases. It may be confounded with aneurism of the carotid artery, with scrophulous enlargement of the lymphatic glands, with encysted and sarcomatous tumours of the trachea and its vicinity, with dilatation of the internal jugular vein, and perhaps with other complaints. From aneurism it may be distinguished, in general, by want of pulsation, by the comparative insensibility of the tumour, by the softness of its texture, by its mobility, and by the circumstance of the swelling accompanying the motions of the larynx and trachea, when the patient is desired to imitate the action of swallowing. But sometimes the goitre is so large, and is so identified with the adjacent cellular texture, that little or no movement of the trachea can be observed. When goitre is extensive, and occupies one side of the neck only, and when, at the same time, there is a pulsation in it from the enlarged and varicose state of the vessels, we shall not find it always easy to discriminate between it and aneurism. Occasionally a pulsation is communicated from the carotid to a goiterous tumour, which happens to lay over it. One instance is noticed by Burns, where the carotid was deeply imbedded in the substance of an enlarged thyroid gland. " The carotid artery being placed," says he, " in the body of the tumour, is neither very rare in occurrence, nor very difficult to explain. It is, indeed, a natural consequence of the extension of the tumour laterally; yet it will not happen in every tumour: it will only occur in those cases, where the consistence of the morbid parts is soft. When the tumour is firm, it pushes the artery, nervus vagus, and internal jugular vein aside. When it is soft, these, as in the present instance, sink into its substance."[*] In most instances of aneurism, however, the carotid is deeper seated than bronchocele, and the pulsation so strong as scarcely to be mistaken. Notwithstanding this, cases have been related where the most able surgeons have found it impossible to offer a decided opinion. A Creole negro had a tumour on the neck, which was submitted to the inspection of some of the most celebrated surgeons in America, Paris, and London; all of whom pronounced the disease an aneurism of the carotid artery; but it was afterwards ascertained, by Boyer, that no such disease existed—but simply an extensive enlargement of the lymphatic and other glands of the neck.* Dr. Samuel P. Griffitts, a distinguished practitioner of this city, has furnished us with an interesting history of a tumour of the neck, bearing so strong a resemblance to carotid aneurism, as to be mistaken for the disease by himself, Drs. Chapman and Morgan, and the late Dr. Dorsey. Upon dissection by Dr. Parrish, it was distinctly ascertained that the carotid was free from disease, and that the tumour was composed entirely of the thyroid gland.

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[*] Surgical Anatomy of the Head and Neck, page 224.

"It was elongated," says Dr. Parrish, "and had obtained a situation directly over the carotid artery; the patient's neck was very short, the pulsation in the carotid was imparted to the tumour laying over it; and, I am informed, there was a strong resemblance to the aneurismal jar or thrill. We are aware, that in dropsy of the chest and pericardium, the heart often palpitates most violently; and this morbid pulsation may explain the throbbing of the carotid, which bore so strong a resemblance to aneurism."† One circumstance which deceived Dr. Griffitts, was the impossibility of drawing the tumour from the artery. "I had frequently endeavoured," says he, "to remove with my fingers, the tumour from the artery, wishing to think the disease was glandular, but could not succeed, as the tumour was so firmly fixed over the vessel, as not to be moved from it; and the pulsation was such as to convey the idea that there was no intervening substance." Under ordinary circumstances, this plan of drawing the tumour from the artery, is excellent. It was by relaxing the muscles of the neck, and separating with the fingers the tumour from the artery, that Boyer was enabled, in the case referred to, to discriminate between the disease and aneurism. By similar means, I have often succeeded in distinguishing enlarged glands and other tumours situated over large arteries, in different parts of the body.

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* Dictionnaire des Sciences Medicales, vol. xviii. p. 541.

† Eclectic Repertory, vol. ix. p. 120.

Many writers have confounded goitre with scrophula: but there would appear to be no legitimate foundation for such a conclusion. In scrophula, the lymphatic glands of the neck and other parts of the body are particularly involved; and other marks in the system, too well known to require discription, evince the existence of the scrophulous constitution. These symptoms do not generally accompany the goiterous tumour. Goitre is strictly a local complaint:—scrophula affects the whole system, and appears at a much earlier period of life than goitre. In countries where bronchocele is endemic, the scrophulous are equally liable, no doubt, with others, to the complaint. Persons who remove from settlements where goitre does not exist, into countries where the disease prevails, are subject to it; but on residing again for some time at their original home, the tumour disappears generally in a short time. This is seldom the case with scrophula, which is little influenced by change of climate. The scrophulous tumour is harder to the touch, and more painful than the goiterous tumour. It is more disposed to suppurate than bronchocele; besides goitre is nearly unknown in certain countries, where scrophula is the common disease. In Scotland, scrophula is almost universal,—goitre hardly ever met with. In Switzerland, goitres are very common, and affect all classes of society, while scrophula is very rare. It is possible for an enlarged thyroid gland to extend so far beyond its natural boundaries, as to occupy the situation of the lymphatic glands of the neck. Mr. Burns has furnished a very instructive case of this kind, in which it would have been impossible, perhaps, without dissection, to have ascertained the true nature of the swelling. "Beneath the sterno mastoid muscle," says he, "the enlarged gland was lobulated and clustered into small processes, precisely resembling a chain of enlarged concatenated glands. Indeed, had I alone trusted to the impressions received before dissection, I would have been led to believe that the lymphatic glands of the neck were actually swelled, and besides, that several of the conglobate glands, placed behind the sterno mastoid muscle, between it and the trapezius, were also affected; for into that space processes from the left lobe of the thyroid gland extended."*

A dilatation of the internal jugular vein is not an uncommon disease, and may sometimes be mistaken for goitre. It may be distinguished, generally, by its low situation—the swelling appearing just above the sternum. The tumour may also be known from goitre, by its softness and compressibility, by its pulsatory and tremulous motion—by the sudden return of the tumour, when pressure is removed—by more or less turgescence, along the whole course of the vein. I once attended a patient, four or five years of age, with Dr. Jennings of Baltimore, upon account of a large swelling of the neck, the precise nature of which, it was difficult to ascertain for some time. It resembled in many respects the enlarged thyroid, and in others, diseased lymphatic glands, but turned out to be a dilatation of the internal jugular. The interesting case of a tumour of the neck, detailed by Mr. Hey, the nature of which he could not ascertain, I have no doubt, was an enlarged vein, and probably the jugular.† The morbid distension of this vessel has been confounded occasionally, with aneurism of the aorta.‡

An encysted tumour may occupy the anterior surface of the trachea; in many respects it is analogous to goitre—is free from pain—is soft and doughy to the feel—follows the motions of the larynx and trachea, and may attain a considerable size. It extends on the trachea as high as the thyroid gland, and descends behind the sternum. The disease has never been described, I believe, as occupying this situation. I have seen only two or three cases of it. An officer of the army consulted me, some years ago, respecting such a tumour, which had been shewn, previously, to several practitioners, who could not give a decided opinion as to its nature. At first I suspected it to be a goitre, but the patient assured me that it had emerged originally, from behind the sternum, and was occasioned, so far as he could determine, by the pressure of a leather stock, which had been worn unusually tight. This circumstance inclined me to believe, that the tumour had no connexion with the thyroid gland, and determined me to puncture it with a lancet. A thick, yellow, cheesy matter, extremely offensive, and three or four ounces in quantity, was discharged from the wound by pressure. The opening was then enlarged, and a probe could be passed to a considerable distance behind the sternum, and upwards along the trachea. The cavity was filled with lint and stimulating injections were frequently employed. Suppuration was established with difficulty, and the cavity was filled up in the course of two or three months. I met with a similar disease afterwards, in a young woman seventeen years of age, and removed it by the same treatment. The wound, however, remained fistulous for a considerable time, in spite of every remedy used. In both cases, these tumours extended so far upwards, and were so deeply imbedded under the sternum, that any attempt at excision would have been hazardous, if not impracticable.

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* Surgical Anatomy, p. 196.

† Hey's Practical Observations in Surgery, third edition, p. 448

‡ Burns on the Diseases of the Heart, p. 259.

In its natural state, the thyroid gland is found to vary in different subjects. In females, it is larger than in males. It is generally made up of distinct lobules, which are collected into numerous lobes or tuberculated masses, joined to each other by a very fine cellular membrane. Rounded vesicles containing a colourless, but sometimes yellowish fluid, are mixed with the lobes. In many subjects these vesicles cannot be discovered, and the existence of a fluid is ascertained, only by rubbing slices of the gland between the fingers, when a peculiar feeling of viscosity may be observed.* There is no proper investing membrane or capsule to the thyroid gland; but the cellular texture is slightly condensed on the surface, so as to furnish a very thin covering, from which processes proceed internally, and form septa or partitions in various directions. The substance of the gland generally consists of two portions, which are placed on each side of the trachea and larynx, and united to each other by a transverse band or slip of the same substance. Sometimes this band is wanting, and then there are two distinct thyroid glands.* No unquestionable excretory duct has yet been discovered. But small openings or canaliculi, described by Morgagni, Bordeu, Walter, and some other anatomists, have been found on the internal surface of the trachea. These openings uniformly occupy one situation, and are two or three in number. They may be found about the middle of the internal surface of the *first* cartilaginous ring of the trachea. Bordeu, in speaking of this ring, says, "Nous avons aussi remarqué, qu'il est, dans tous les sujets ou divisé par une fente plus ou moins étendue et située vers le devant du cartilage, on perce d'un ou deux, et même de trois trous bien apparens et placés aussi, vers le milieu du cerceau sur le devant, ou un peu à coté."

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* Anatomie Descriptive, par X. Bichat.

"Ces trous nous frappérent la premiére fois que nous les vimes: cétoit à Montpellier, en 1741, en disséquant un larynx auprès du feu; la glande thyroide qui étoit extrémement grosse, étant enlevée, nous trouvâmes le premier cerceau presque osseux, mais assez transparent pour laisser apercevoir, au moyen du feu, les deux trous qui n'étoient recouverts que par des membranes lâches qu' on emporta facilement."

"Aprês bien des recherches, on trouva un sujet mort de morte violente; nous examinâmes d'abord la face posterieure du cerceau de la trachée, sans avoir touché la thyroide; la membrane interne de ce cerceau étoit pleine de petits trous difficiles à apercevoir; nous introduisimes des soies dans cinq de ces trous, et en les conduisant légèrement, elles allérent se rassembler en deux endroits, trois dans l'un et deux dans l'autre; ces endroits étoient précisement les deux trous du cartilage; ces soies allérent, en les

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* Soemmering de corp. hum. fabric. vol. 6. p. 39.

poussant, se perdre dans la glànde. M. Barbuot, medecin de Semur, étoit présent à cette opération."* From a perusal of these and other passages in Bordeu, some years ago, I was induced to examine the openings described, in a great many subjects, under an impression that they were the mouths of excretory ducts from the thyroid gland. To ascertain this, I made a number of experiments with the mercurial injecting apparatus, the small pipes of which were introduced directly into the openings in the cartilage, and found that the mercury sometimes passed with facility through these small canals, but met with resistance when it reached the thyroid gland. In three or four instances I succeeded in pushing it to a considerable distance under the cellular covering of the gland, and even among the cellular texture into the substance of the gland, as I afterwards ascertained by cutting it open. But in all probability the mercury passed, in each case, from rupture of the cellular tissue, and did not follow the natural course of the duct. I was assisted in these experiments by my very intelligent friend, Mr. Horace H. Haydn, of Baltimore, who is not only highly distinguished in the profession to which he is devoted, but whose comprehensive mind leads him to investigate almost every subject of interest connected with pathology. At his suggestion I endeavoured to find a communication between the thyroid gland and the ventricles of Galen, and with this view introduced the mercury into the bottom of each of these cavities. After several ineffectual attempts, I succeeded in filling the cellular texture of the thyroid, and to a much greater degree than from the openings of the trachea. I mention these circumstances to corroborate the suggestions of Morgagni and Bordeu, that there are passages from the thyroid gland, which serve to deposit its secretions in the trachea and perhaps in other places. The observation may be useful to those who feel disposed to investigate the subject further. In addition, I may state, that Foderé succeeded in blowing air from the larynx into the thyroid gland, so as to distend it considerably. In another instance he filled the trachea with spirit of wine, and upon cutting into the thyroid gland, the smell of the liquor was distinctly perceivable.

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* Œuvres complètes de Bordeu, par Richerand, tom. 1. p. 98.

"Qu'on prenne," says he, "un larynx auquel, cette glande est attachée, bien lavé et nétoyé avec une légeré dissolution de potasse, et ensuite séché, qu'on en bouche exactement l'extremité inferieure, puis qu'on adapte au trou de la glotte, un tube contigu à une vessie pleine d'air, et qu'on lutte bien l'appariel; en comprimant la vessie, on verra la glande thyroide augmenter de volume."

"La même éxpérience réussit, quoi qu'à un moindre degré, avec l'alcohol. En coupant la glande après avoir comprimé la vessie, on sent distinctment l'odeur de ce fluide."[*]

Lalouette discovered an immediate connexion between the thyroid gland and the lymphatic vessels which pass along the thyroid and cricoid cartilages.[†] Many cases have been recorded by different writers, where an enlargement of the hyroid gland has been suddenly produced in consequence of violent exertions of the muscles of the neck in lifting heavy weights, or in consequence of laborious efforts of the patient during protracted and difficult parturition. It has been maintained, also, that goitre is produced among the inhabitants of certain European districts, from the habit, which is frequent among the lower order of people, of dragging burthens up the hills by cords tied round the upper part of the chest. According to Mr. Heckewelder, who often met with goitre among the American Indians, the disease never made its appearance among the girls until they began to carry heavy burthens on their heads.[‡] These circumstances would favour the idea of Bordeu, Foderé, Morgagni, and others, of the existence of a direct communication between the trachea and thyroid gland.[§] Many theoretical uses have been assigned to the thyroid gland besides those already mentioned. It does not come, however, within the scope of my purpose to detail them.[||]

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[*] Traité du Goitre et du Cretinisme, p. 58.

[†] Haller Elementa Physiologiæ.

[‡] Barton's Memoir, p. 46.

[§] See Morgagni's Adversaria, 5. p. 66.

[||] Those who wish for information on the subject may consult Haller's Elementa Physiologiæ, lib. 9. p. 22. Soemmering de corp. hum. fabr. vol. 6. p. 41. Coxe's Museum, vol. 3. p. 27.

When a goiterous tumour is examined by dissection, several circumstances are presented worthy of notice. One or both lobes, and sometimes the middle lobe or isthmus of the gland, are found enlarged beyond their natural boundaries. Upon cutting into their substance, the texture is found more or less compact, intermixed with numerous cells, containing a transparent glutinous liquor, which may be drained off in such quantity, by pressure, as to reduce considerably the bulk of the tumour. These cells vary in size; some being large enough to contain a pea, while others are exceedingly small. The fluid they contain becomes a solid transparent jelly, when the gland has been immersed for some time in proof spirits.* Although the thyroid gland in its natural state is abundantly supplied with large arteries, yet its capillary vessels are comparatively few, and the quantity of blood determined to its substance not so great as commonly supposed. In bronchocele all the vessels are greatly enlarged and varicose, and the quantity of blood materially increased, as is evinced by the throbbing of the tumour during life, and by injection of it after death. This preternatural accumulation of blood so frequently accompanies the kind of diseased enlargement of the gland which I have described, as to cause some writers to rank it as a particular species of goitre,—denominated *sanguineous goitre*. This distinction, as well as every other division of the disease into species, is, perhaps improper, inasmuch as the appearances presented on dissection are never sufficiently uniform to enable us to characterize with precision each morbid change of structure. It is certain, however, that an unusual determination of blood generally accompanies the structure I have described,—and which nosologists have called the *sarcomatous bronchocele*. In all probability the other species mentioned are but varieties of this common and perhaps original form of the complaint.

Sometimes the texture of the goiterous tumour, instead of being compact and solid, is soft and spongy, and large cavities or membranous vesicles are dispersed throughout, which contain a thin, limpid, or serous fluid. This has been called the *encysted*, *serous*, or *watery bronchocele*. It is a modification only of the common disease; for sometimes the fluid changes into a yellow, tenacious, and meliceroous matter. The cells of the thyroid gland are said to have been filled occasionally, with hydatids; but such appearances may have been confounded with the watery collections just described. The *bronchocele ventosa* cannot be considered as a variety of goitre, but only an emphysematous tumour of the gland, or of the adjacent cellular texture.

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* Baillie's Morbid Anatomy, p. 86. Also Engravings, Fasciculus, 2. p. 25.

It is not uncommon to find in the substance of goitres of long standing, bony particles and even considerable masses of ossified matter. Several examples of this kind are mentioned by Bonetus, Morgagni, Kerkringius and other old writers. Dr. Baillie, in his morbid anatomy, speaks of the thyroid gland being sometimes converted, in old people, into a bony mass. In this respect the disease nearly resembles other sarcomatous tumours, in which we are accustomed to meet with cartilaginous and ossified productions. Calcareous concretions are said to have been discovered, in the substance of the thyroid gland, affected with goitre.* Pieces of tuft stone have been removed from the thyroid gland, in several instances, by a Swiss surgeon.†

The substance of bronchocele is seldom converted into purulent matter. But cases are recorded by Petit and Hevin, where spontaneous cures were affected in this way. Severinus relates a case, in which purulent matter was discharged from a bronchocele, mixed with a substance resembling charcoal.‡ Dr. Baillie has given a drawing of a preparation contained in the Hunterian cabinet, where an abscess formed in the right side of the thyroid gland and afterwards communicated by ulceration with the trachea, so as to suffocate the patient.§ Alibert relates the case of a patient in the hospital of St. Louis, who laboured for years under an enormous bronchocele,* and was eventually relieved of his burden by suppuration taking place in its substance. Ulceration was spontaneously established and upwards of five pounds of purulent matter discharged.*

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* Haller's Elementa Physiologiæ, Vol. III. p. 400.

† Coxe's Travels.

‡ De Recondita Abscessuum Natura, p. 19‡

§ Series of Engravings, p. 2⁻

Burns gives an instance, where suppuration took place in both lobes of the thyroid gland. The matter was slowly secreted and the integuments became gradually distended, until they formed a large pouch which hung over the sternum and contained several pounds of pus. The sides of the cyst united, and the patient was ultimately cured.† In speaking of abscess, following bronchocele, Portal remarks, that the cartilages of the larynx and rings of the trachea, are sometimes eroded by caries. "On a trouvé dans des sujets qui étaient morts de suffocation, les cartilages thyroide, cricoide, et les anneaux cartilagineux de la trachée artére, rougés par la carie, à la suite d'un abscess dans la thyroide. Valsalva, Morgagni, Lieutaud ont cité de paréils exemples dans leurs ouvrages."‡

I have had three opportunities of dissecting goiterous tumours. The first was in a man upwards of sixty years of age, who had laboured under a very large and tuberculated swelling of the thyroid, almost from infancy. The tumour occupied both sides of the trachea, and was very solid, and insensible to the touch. It produced very little inconvenience, and the patient died of another complaint. Upon dissection, I found each of the enlarged lobes completely sarcomatous, without any membranous vesicles, or fluid, except a thick yellowish, lardaceous or oleaginous matter, in small quantity, which could be pressed, by force, from the diseased mass. Fibrous bands, similar to those which occur in the scirrhous breast or testicle, intersected the tumour in various directions. The larynx and trachea were not altered in structure, but the mouths of the small muciparous ducts, which open on the lining membrane of the trachea, were not perceptible; and the *tracheo-thyroideal* passages of Bordeu appeared less conspicuous than usual.

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* Nosologie Naturelle, p. 467.

† Surgical Anatomy, p 188

‡ Cours d'Anatomie Medicale, tome iv. p. 564

About two years ago I had an opportunity of inspecting the body of a woman thirty-five years of age, who died of apoplexy. A large tumour occupied the left side of the thyroid gland. Upon turning aside the sterno-thyroidei and omo-hyoidei muscles the gland was brought into view and presented an immense number of varicose veins distributed over its surface; all the parts in the neighbourhood, seemed vascular in the extreme. Upon opening the gland a considerable quantity of thin greenish fluid was discharged, and the small cells which contained it, were distinctly perceived. The right side of the gland, together with the isthmus, was slightly enlarged; but in other respects appeared to possess its natural structure. This woman, as her husband informed me, had suffered occasionally from inordinate pulsation in the tumour, and from its pressure on the trachea, but in general, experienced little inconvenience. She was a native of Holland, where the disease commenced about the tenth year of her age, and had slowly increased. It appeared to have had no connexion with the complaint of which she died.

With the history of the third subject, I am unacquainted. It was a girl about fourteen years old, who was much emaciated, and had laboured, apparently, for a considerable time, under constitutional disease. Both sides of the thyroid were involved; but the disease was evidently in its commencement. The vessels of the gland were slightly varicose, and the cellular structure of the interior, contained a small quantity of transparent fluid. The larynx and trachea were unaltered, and the small openings of the first cartilaginous ring were distinctly observable.

The causes of goitre are involved in much obscurity. This will account for the numerous, diversified and contradictory speculations on the subject. By many, the disease has been attributed to the use of particular alimentary substances, especially poor and unwholesome diet;—to the drinking of cold or snow water, or water strongly impregnated with limestone, or other calcareous matters; the immoderate use of spirituous and vinous liquors, debauchery, the repulsion of cutaneous diseases, and many similar explanations have likewise been resorted to; all of which are too hypothetical, and so frequently contradicted by facts, as to deserve no attention. It is certain, that goitre prevails as an endemic disease chiefly in countries where the atmosphere is loaded with moisture, in vallies enclosed by lofty mountains, and which are exposed to the direct and reflected heat of a powerful sun. In some of these vallies, the fogs are visible every morning, rise with the sun in a thick body, and seldom disappear entirely, until the afternoon.* It is, however, well ascertained, that those persons who do not reside in or near the vallies where goitres prevail, but live on the sides or tops of the adjacent mountains, do not labour under the disease. Again, it is a fact established beyond all doubt, that the mere removal of a goiterous person from the valley where he acquired his disease, to the top of the contiguous mountain, will diminish the size of the tumour, and in time, remove it entirely. The same observations, perhaps, to a limited extent, may be applied to cretinism, a disease so often concomitant, but probably independent of goitre. "All the cretins that I saw," says Dr. Reeve, "were in adjoining houses, in the little village called La Batia, situated in a narrow corner of the valley, the houses being built up under ledges of the rocks, and all of them very filthy, very close, very hot and miserable habitations. In villages situated higher up the mountains, no cretins are to be seen, and the mother of one of the children told me of her own accord, without my asking the question, *that her child was quite a different being* when he *was up the mountain, as she called it, for a few days.*†" Frequent opportunities are offered in this country, of observing the effects produced on goitre, by the removal of individuals afflicted with it into districts where the disease is unknown. Numerous cures, of very large goitres, have been effected in persons, who acquired the complaint at Pittsburg—simply by spending a few months in Philadelphia, or other distant places. These circumstances indicate, beyond doubt, something peculiar in the atmosphere or in the exhalations from vallies or other places where goitre is found. That the disease, at all events, is not owing to poor living or to the drinking of snow water, is sufficiently proved by the circumstance of its not prevailing in certain countries where the inhabitants are accustomed to subsist on a very meagre and scanty diet. Besides,—the rich inhabitants of the Vallais and of the state of New York, are equally subject to the complaint with the poorer people. In Greenland, and Lapland, where the inhabitants use snow water almost entirely, there is no goitre, while in Sumatra, in Bambarra, and in many other warm countries, where snow is never seen, the disease is very common.*

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* Marsden's History of Sumatra.

† Account of Cretinism, Edinburgh Journal, vol. v. p. 35.

In those parts of the state of New York, New Hampshire and Vermont, particularly along the course of the Connecticut river, where goitres prevail, it is remarkable that the disease is most common in those places which are covered with wood and are uncultivated; but in proportion as the country is settled and the lands cleared, the disease is found to decline, and in many places is already nearly extinct.† In 1798 bronchocele was so common at Pittsburg that out of 1400 inhabitants, not less than 150 had the disease.‡ Since that period the complaint has so much declined, in the same place, that it is said very few now labour under it. The change is usually attributed by the citizens of the town, to the general introduction of coal fires. All these circumstances tend to show that bronchocele is produced by a peculiar atmosphere, or by certain morbid exhalations from marshes or other grounds. What the peculiar nature of this exhalation is, we have no more means of ascertaining, than we have of finding out the constituents and mode of action, of marsh miasma, or of the agents which create yellow fever, or any similar disease. The late professor Barton imagined goitre to arise from the same causes which produce intermittent and bilious fevers.* But it has been ascertained that goitre originates in many districts where intermittents are unknown, and intermittents on the other hand are frequent where goitre has never been seen. Along the shores of the Delaware and Chesapeake bays, intermittent and bilious fevers universally prevail, and goitre is never found. It is possible, however, that the cause of goitre may be allied to that of intermittent, but so modified by particular circumstances, which we shall probably never be able to ascertain, as to produce very different effects. But those who are anxious for information, respecting all the supposed causes of goitre, should consult the work of Foderé, Coxe's travels, Saussure's voyages, Gautieri de Tyrolensium struma, &c.

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* Vide Marsden, Park's Travels, Humboldt and Bonpland, &c.

† Mease's Observations.

‡ Barton's Memoir.

Whatever may be the remote physical causes of goitre, I am inclined to believe, that the disease arises immediately from an obstruction of the *tracheo-thyroideal* passages of Bordeu, of the openings, communicating with the sacculus laryngeus and the thyroid of which I formerly had occasion to speak, or of other passages with which we are unacquainted. I am inclined to draw this conclusion from the circumstance of a watery fluid being found to occupy naturally, the cells of the thyroid gland—from this fluid being increased in quantity in almost every goiterous tumour, and from the passages of Bordeu being much smaller in the first dissection I made of bronchocele, than they are usually met with in subjects without such disease. *This is a mere conjecture.* Neither is it original—but was advanced by one of the older writers on surgery. I mention it, merely to induce those who have frequent opportunities of investigating the structure of bronchocele by dissection, to attend to the appearances of these passages and to endeavour to discover other communications with the gland.

I shall next make some observations on the cure of goitre. During the last nine or ten years, I have had frequent opportunities of observing goitre under different forms at Baltimore and the adjacent country, where accidental cases of the complaint are by no means unfrequent. Besides—I have been in the habit of treating patients from Pennsylvania, Virginia and other distant places, where the disease is endemial. I have resorted to all the usual remedies, sometimes with advantage, at other times, without any benefit whatsoever. In the commencement of my practice, I employed the burnt sponge in the form of powder. mixed with honey and other materials,—the lozenges of Ring, which consist of cinnamon, gum arabic, syrup and burnt sponge mixed—the simple decoction of the sponge, as recommended by Herrenschward of Berne, in Switzerland,—but generally without any decided effect.* In two instances, I succeeded in removing small goitres by the use of sulphate of potash, continued for several weeks in large doses. This remedy is said to have effected many cures, in the hands of Foderé and other practitioners. The different preparations of mercury and antimony, muriates of lime and barytes, I have tried without the slightest advantage. In one case after the inefficacious exhibition of many of the remedies mentioned, my friend Dr. Cromwell, of Baltimore, effected a complete cure in a patient upward of twenty years of age, who had laboured for some tim under a goitre, by the repeated application of blisters. M Benjamin Bell says, that he arrested the progress of a ver large bronchocele by the same means. Stimulating frictio with flannels, immersed in camphorated liniments, and othe applications of a similar nature, are commended by Underwood. Foderé remarks that he frequently cured smal Spanish dogs of goitre by the same means, at Maurice where these animals are very subject to the complaint.† Frictions with mercurial ointment and various stimulating plasters have likewise been extolled by different writers. Boyer has employed for many years cataplasms, or bags of emollient herbs, applied directly to the tumour, and worn night and day for weeks or months, and often with success. Compression has sometimes been found serviceable by Foderé; and Dr. Physick once succeeded in effecting a complete cure of the disease, in a lady of this city, by keeping up a continued but moderate pressure by means of a bandage for several months. Mr. Holbrook, a surgeon of Monmouth in England, where goitre is endemic, has cured a number of patients, by the use of steady pressure, after the failure of other remedies.*

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* Vide Barton's Memoir—also Caldwell's Medical and Physical Memoirs, p. 279.

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* For an account of the use of these remedies see Foderé, p. 110. Ring in the 4th, 5th, and 11th volumes of the London Medical and Physical Journal.

† Foderé, p. 115.

But I have found no general or local remedies so efficacious as the extract of Cicuta. Indeed for several years I have depended solely upon the use of this medicine, and may declare confidently, that I have seldom had recourse to it in the early stage of bronchocele, without some benefit. Accident alone led me to the use of it. A young lady of sixteen years of age had laboured for two or three years under goitre, which occupied each side of the larynx and trachea, and was considerably larger than her two fists. I attended her for several months, and employed every remedy likely to prove of service—but in vain. The disease still continued its course unrestrained, and had become extremely troublesome from interrupting the breathing. Finding every thing useless, and growing tired of importunity, I gave her a box of cicuta pills, which had been prepared for another purpose. She returned in about ten days, with the tumour diminished at least one fourth in bulk. I was astonished at the change, and immediately furnished her with an additional supply of the medicine, which she took in large doses, and with such decided effect, that at the end of the third week no vestige of the tumour remained. This lady was a native of Baltimore, and had never resided in any other place. For several years before the commencement, and after the removal of her complaint, she lived at the upper extremity of Howard-street, in a remarkably healthy situation. From this period I had frequent opportunities of using the cicuta, and, under favourable circumstances, almost invariably with success. I found by experience, that from the age of ten or twelve years up to twenty, and in cases where the goitre was large and spongy to the touch, and had not existed very long, that the cicuta was almost a certain remedy; but on the other hand, when it occurred in adults beyond the age mentioned, and in old people, that although it sometimes diminished the size of the swelling, yet, in general, it was productive of no advantage. The cases which I had an opportunity of treating, were confined chiefly to the city of Baltimore and its vicinity, and to several of the counties of Maryland where goitre does not prevail as an endemic, but is frequently seen as an accidental disease. I could therefore better judge of the value of the remedy than I could from exhibiting it to those patients who removed from their homes where the disease was endemic—inasmuch as a simple change of residence, as I have before mentioned, is oftentimes sufficient to effect a cure. Among such persons, however, its effects appeared to be equally salutary. With regard to the particular preparation employed, I may remark, that the extract, as prepared by Mr. Edme Ducatel, of Baltimore, is the only medicine of the kind that has proved useful. I have often used the imported article, and the medicine as prepared by other druggists in this country, with no advantage whatever. Mr. Ducatel cultivates the plant upon a large scale in his own garden. With his particular mode of preparing the extract I am unacquainted; but as he has obligingly promised to furnish me with the process, I expect to communicate some information on the subject, at a future period. I have generally commenced with two grains of the medicine once in twenty-four hours. In some cases I have gradually increased the dose to fifty grains during the same period, according to the effect produced. Where giddiness or stupor has followed the exhibition, I have diminished the dose, or abandoned the medicine for a short time. I have derived much benefit from the use of cicuta in other diseases besides goitre, where the European or common preparation proved of no service.

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* London Medical Repository, vol. 8, p. 288.

These facts are detailed, in hopes that practitioners who have ample opportunities, in districts where goitre prevails, of observing the disease, may put the remedy still further to the test. It is very doubtful, however, whether the cicuta, or any other remedy, will exert such influence as to effect a radical cure, or even alleviate the complaint, so long as the causes which produce it still continue to operate. I hope soon to ascertain this point, as I am now giving the medicine a trial in the family of a gentleman in the state of New-York, all of whose children, and many of whose neighbours, are affected with goitre. It is possible that cicuta may have been employed before in the treatment of this disease, but I have not been able to meet with any mention of it.

Bronchocele sometimes disappears spontaneously. Occasionally the tumour is removed suddenly. Several examples of this sort are related by Alibert. In one instance, during the French revolution, a woman was seized with a fit of melancholy, and a large bronchocele, from which she had suffered exceedingly, disappeared with the utmost rapidity.*.

Bronchocele may become so large as to endanger suffocation. In this case, *extirpation*, an operation first recommended by Celsus, has been resorted to, and sometimes with success. But such favourable terminations are extremely rare—as many of the older, and some of the modern French and English surgeons have sufficiently proved. Palfin, in his Surgical Anatomy, relates the case of a lady of rank in Paris, who perished from hæmorrhagy, in consequence of the removal of a large goiterous tumour, by an adventurous surgeon, who undertook the operation in spite of the remonstrances of some of the most experienced practitioners. The operator had scarcely left the house, before the hæmorrhage broke out, with tremendous violence, and destroyed the patient in a few minutes.* Gooch mentions three cases, in which the operation was performed contrary to his advice, and that of some other surgeons. Two of the patients lost their lives from hæmorrhage, and the third was only saved by constant pressure, kept up day and night for the space of a week, by the fingers of several persons employed for the purpose.† Mr. John Bell mentions more than one instance, where surgeons have opened tumours arising from or connected with the thyroid gland, from which the flow of blood has been so copious, as to oblige them to abandon the operation and close the wound as speedily as possible.‡ Dessault, upon one occasion, undertook to remove a bronchocele from the neck of a woman, but the blood issued with such rapidity, as to force him to stop and endeavour to secure the vessels by throwing a ligature around a considerable portion of the tumour which she had dissected up. The patient died in a very short time, in convulsions, occasioned, it was supposed, by irritation from the ligatures.§ Another case is recorded of a patient who had a large bronchocele, which was productive of no inconvenience to him, removed by the knife, by the advice of Dessault, and contrary to the opinion of Baron Percy and Louis. It was found impossible to stop the blood and the gentleman expired almost immediately. Many examples of a similar nature are recorded by Bonetus, Severinus, and others of the older writers. But perhaps, the most remarkable operation of the kind to be found in the annals of surgery, was executed, a few years ago in Paris, by the celebrated Dupuytren, on a young woman who had laboured for a great length of time under an enormous bronchocele, which interrupted respiration and deglutition to such a degree, as almost to destroy her. The patient had presented herself frequently at the Hotel Dieu, in expectation of having the tumour removed by the knife. At several consultations held on her case by the most distinguished surgeons of Paris, it was unanimously determined, that no operation could be performed with any prospect of success. The tumour, however, still continuing to increase in size, and the entreaties of the patient for its removal becoming more urgent, Dupuytren, at last, consented to engage in the undertaking. By slow and cautious dissection he succeeded in detaching the left side of the tumour, without dividing any of the large arteries, veins or nerves of the neck. The thyroid arteries, in particular, which were very much enlarged, were each drawn out and tied with two ligatures before they were cut. The right portion of the tumour was then removed, in a similar manner, and the whole operation completed, with the loss only of a few spoonsful of blood. But the patient suffered extremely, as the operation was necessarily protracted, and the dissection carried on among parts of the utmost delicacy and sensibility. She never recovered from the shock communicated to the nervous system, and expired in thirty-five hours after the operation.*

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* Alibert's Noveaux Elémens de Therapeutique.

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* Anatomie Chirurgicale, tome ii. p. 313.

† Chirurgical Works, vol. iii. p. 158.

‡ Principles of Surgery, vol. iii.

§ Dictionnaire des Sciences Medicales, tome xviii. p. 556.

It would appear, from these details, that the extirpation of even a small tumour of the thyroid gland, is attended with difficulties which should dismay the most expert and enterprising surgeon. Cases are recorded, however, where operations of this kind have been attended with success. Fodéré states that a barber relieved his wife of a very large bronchocele by excision. The same author mentions two other instances within his own knowledge where Giraudi, an adventurous surgeon of Marseilles, succeeded in curing his patient, by the same means.† Dessault dissected out the right side of an enlarged thyroid gland of a female patient in the Hotel Dieu, who recovered without a bad symptom in a month after the operation.‡ Two cases of successful extirpation of bronchocele are detailed by Dr. Harris. In the first case the tumour was not larger than a pullet's egg, in the second it was of considerable size. "About three months since," says Dr. Harris, "an application was made to me by a lady from South Carolina. I think the bronchocele was full as large as any I had ever seen. The tumour had been increasing for twenty-two years. It extended from the chin, which it buoyed up, along the trachea, until it descended an inch, or perhaps more, under the breast bone, and spread laterally a medial distance to each ear."* No hæmorrhage of consequence followed either operation, and both patients recovered in a very short time. There is great reason, however, to believe, from the histories of these cases, that the tumours neither constituted a part of the thyroid, nor were connected with it. But from all the information I have been able to collect, on the subject of the removal of this gland by the knife, I am inclined to believe, that less danger would attend its excision, as respects hæmorrhagy, than is commonly imagined. Our knowledge of the means of arresting hæmorrhage has been greatly improved within a few years; and it will be seen, that Dupuytren, by adopting a practice long ago recommended and employed with the greatest success, in this country, by Dr. Physick (the tying of large arteries before they are cut) was enabled to extirpate an enormous bronchocele, with the loss of only a small quantity of blood. But I very much question the propriety of attempting the removal of a goiterous tumour, in any case, inasmuch as I conceive that hæmorrhage is less to be dreaded than the inflammation and irritation which follow the operation. Under ordinary circumstances, the tumour may attain a very large magnitude, without endangering the patient's life or producing much inconvenience; and if it should increase to such a degree as to render death inevitable, there is very little probability that the patient can be saved by so severe an operation as must necessarily be encountered.

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* Dictionnaire de Sciences Medicales, also Pelletan's Clinique Chirurgicale. vol. 1. p. 215.

† Traité du Goitre, &c. p. 118.

‡ Surgical Works, vol. 1. p. 257.

But although I would not recommend the excision of the thyroid gland, there is another operation to which I would not hesitate to resort in case of necessity. I allude to the tying up of the great arteries in the vicinity of the tumour, from which it chiefly derives support. Mr. Thomas Blizzard, of London, was the first, I believe, to execute the operation. The thyroid arteries, on each side of the neck, were included in a ligature, and the tumour diminished, in in the course of a week, one third in size. The patient, however, did not recover, but died from repeated secondary hæmorrhage, occasioned by an attack of hospital gangrene. But he lived long enough to evince the propriety and practicability of the operation.* In 1818 a similar operation was performed by professor Walter, of the University of Landshut, on a man twenty-four years of age, who had an enormous bronchocele, from which he suffered extremely. The inferior thyroideal artery of the left side was taken up, and at the end of a fortnight the left portion of the tumour had diminished so much in bulk, as to induce the operator to include the superior thyroid of the right side in a ligature. No inconvenience followed either operation, and the bronchocele, in a short time, almost disappeared, leaving behind only the elongated skin, which hung from the neck in the form of an empty sack.†

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* New-York Medical Repository, vol. 11. p. 242.

Art. VI. *An Essay on the Rupture of the Uterus.*

By William P. Dewees, M. D.

THERE is great reason to believe that the uterus is ruptured during parturition much more frequently than is commonly supposed. Many of the sudden deaths during labour or quickly after, are entirely unaccounted for, though the symptoms attending the illness justifies the belief, that they were from this cause. At this we need not be surprised, when we reflect how few are properly qualified to practice midwifery; many male, as well as female practitioners, being entirely ignorant of the first and most common principles of the art, and consequently wholly incompetent to appreciate the danger, or determine the nature of the accidents which may accompany, or speedily follow the process of parturition.

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* Burns's Surgical Anatomy, p. 202.

† Bulletin de la Société Medicale d'Emulation, 1818.

But cases of this kind are not concealed by ignorance alone. Not a few have been hidden, lest the disclosure might injure reputation. This want of candour has been perpetuated by two causes; the first proceeding from the express recommendation of the otherwise liberal Dr. Smellie, and the second by the unsettled state of the questions, "what, or if any thing, should be done for the relief of the patient, when this melancholy accident has taken place."

Dr. Smellie* says, that in a case he was called to, "on introducing the hand to deliver the placenta, the uterus was found torn at the fundus, and the intestines pushed down." "In order to avoid reflections, this accident was kept secret." And, in p. 386, a correspondent tells him in a similar case, that "according to his *prudent* advice, he spoke nothing of the matter." Here then is high authority for the concealment; and it is be feared that too many have availed themselves of it.

But, the unsettled practical point, has led to more numerous concealments than the first—for while the practitioner is balancing in his mind the propriety or impropriety of acting, the patient expires; and then the fear of injury to his reputation, should he candidly make known the state of the case, forever seals his lips. To settle then this agitated question, is the principal object of this paper. We pretend to but little novelty in this discussion; our chief aim is to remove, as far as may be in our power, all ambiguity from the subject, by reducing our duty to a few, but important rules.

In prosecuting this object, we will first enquire whether it is proper to attempt any thing for the relief of the unfortunate woman who may have suffered this accident? Secondly, enumerate the causes of it, with their mode of action.

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* Vol. iii. p. 386.

Thirdly, detail the symptoms and consequences of the rupture. Fourthly, point out the mode of proceeding, under all the various circumstances with which this accident may be complicated.

1. The late Dr. Hunter considered it an act of wanton cruelty, to disturb a patient who had suffered this accident, by attempts to deliver her—so inevitable did he consider her death; and so imposing was his authority among the British and American practitioners, that it became an undisputed point of doctrine, and to have departed from it, would have been considered a species of murder. This opinion was founded in error, and perpetuated by the force of education; and its propriety was not seriously called in question in Great Britain, until about the year 1784; at this time a case fell under the notice of Dr. Douglass,* who *dared* to differ from the established authority, by delivering his patient, and was rewarded for his good sense and zeal, by her recovery. Examples of this kind had occurred on the continent of Europe, but they had failed to make the impression they deserved. The medical public are much indebted to Dr. Douglass for daring to dissent from the almost universal opinion of his fellow practitioners; and he has taught them by his intrepidity, what they failed to learn from the experience of the continental accoucheurs, that the death of a woman who had suffered a laceration of the uterus, is not inevitable. The case just noticed made for a time a strong impression; but the infrequency of the accident, and the force of preconceived opinions, but too soon effaced it.

When a case occurred, authority was sought for in the writings and opinions of men whose influence and credit was greatest with the medical public; and they were found but too often consonant with those promulgated by Dr. Hunter and some other teachers of high character: hence we find, that "not to give pain where we could not do good, became an aphorism," in the treatment of the rupture of the uterus.

It is truly a matter of surprise, from whence this opinion originated; as it had neither reason nor fact to support it—it was taken for granted, what remained to be proved, or perhaps more properly, it was manifesting a total disregard of facts, as incontestable in their nature, as important in their consequences. Had Dr. Hunter, and others who espoused his notions, never heard of the recovery of the woman, where delivery had been resorted to, we should be less surprised; but as we cannot suppose them ignorant of what had happened in other countries, we are totally at a loss to account for their opinions on this subject.

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*Essay on Rupture of the Uterus, p. 7.

We cannot withhold our astonishment, when we receive from Dr. Denman* the following declaration; and the more especially, as his extensive practice and reading should have led him to contrary conclusions. "If the principle on which all operations in surgery are justifiably performed, be to lessen or to remove present danger or suffering, or to prevent greater, though distant mischief; it may be reasonably asked, what benefit can result to the patient or to society, or what credit to the profession, from an operation by which her present sufferings are in some degree aggravated, and by which neither the lives of the parent or child were ever known to be preserved, with whatever knowledge or dexterity such operation may have been performed? If we further take into the account, that the few instances of recovery or escape from danger, when this deplorable accident has happened, (if there be no error in the accounts transmitted to us) were those in which no operation was performed, we may be warranted, without presumption, in coming to the following conclusion; that,

"*When the uterus is ruptured at the time of labour, both reason and experience show, that the patient has a better chance of recovering, by resigning the case to the natural efforts of the constitution, than by any operation or interposition of art.*

"If those deductions be legal, and this conclusion just, in cases of the rupture of the uterus, it will follow, *a fortiori*, that in lacerations of the vagina, the patient ought not, for that reason, to be delivered by art."

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* Essay on the Rupture of the Uterus, p. 17 and 18.

We have already expressed our astonishment at these declarations of Dr. Denman, because they are in defiance of both "reason and experience," and directly contradicted by the testimony of the most respectable authorities. And the premises and conclusions are the more faulty, as no proofs are offered of their validity. We shall therefore prove, that there are instances of recovery from this deplorable accident, where there was an "interposition of art," by relating cases from authors of deserved faith, and challenge the advocates of Dr. Denman's opinions to prove, that there ever was an instance of recovery, "where no operation was performed."

The first instance that is distinctly recorded, is that mentioned by Heister,[*] on the authority of a surgeon named Rungius. In this case, the intestines were distinctly felt through the rupture of the uterus, and through which the fœtus was extracted; yet the the woman recovered. Dr. Douglass, in his Essay,[†] gives the history of Mrs. Manning, who also recovered. Dr. Hamilton[‡] relates another instance of complete and entire recovery, although the intestines issued through the wound of the uterus, and were reduced by him after the delivery of the child. In this case he declares, "the recovery was nearly as good as if no extraordinary accident had happened." Dr. Ross relates the case of a Mrs. Granan of Eppendorf, near Hamburgh, who suffered this accident in two consecutive labours, and yet recovered.[§] Mr. Kite gives a case of ruptured uterus, which terminated favourably.[||]

In a copy of the MS. lectures of Dr. J. Hamilton, the present Professor of Midwifery in Edinburgh, there are two cases related of recovery, one of which he himself attended, and says, it was "one in which almost every circumstance was unfavourable;" for "in bringing the child through the lacerated part," he "felt the uterus tearing more; the woman lost three pounds of blood, yet she recovered and afterwards had children." The other case occurred in Lancashire; "a poor woman fell from a cart, in consequence of which, the uterus was ruptured, and the child passed into the abdomen; the bones of the pelvis were so much injured by the fall as not to allow of delivery, being much *mashed*, the Cesarian operation was performed, and she recovered."

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[*] Instit. de Chir. tom. ii. p. 137.

[†] Essay on the Rupture of the Uterus, p. 7.

[‡] Outlines, p. 344.

[§] Annals of Medicine, vol. iii. p. 377.

[||] Mem. Med. Soc. of Lond. vol. iv. p. 253. Madam La Chapple also; Annuaire Medico-Chirurg. tom. i. p. 542.

Mr. Thibault* relates a similar case to the one just recited—gastrotomy was performed with the most entire success to the woman, though too late for the preservation of the child.

Baudelocque relates† that a M. Lambron, a surgeon of Orleans, performed gastrotomy twice on the same person, with the desired success to the woman, after the rupture of the uterus. This woman became pregnant a third time, and was delivered naturally of a healthy child.

Mr. Hugo‡ relates a successful case also, and we could without difficulty increase the number; but these are sufficient to prove that success has attended the "interposition of art." The cases we have just cited, were all fortunate to the woman, but the child uniformly perished; this was rather owing to the time at which art interfered, than to the mode it adopted. Of this we have sufficient proof in the case related by Burton.§ He says "I was called to the wife of a broker in the city of York, who had had several children—she fell into labour at the regular time; she had only a slow labour at first, but after some little respite, her pains became more violent, during one of which, she perceived something to crack within her, as she termed it; after which she exchanged her pains for faintings, &c. with an intermitting pulse; on this account I was called in: being told every thing that had happened, I was apprehensive of what, indeed, proved to be the case; wherefore I told the by-standers my opinion; and that, as the child was alive, it was proper the woman should be delivered as soon as possible, which was done directly. The child was small, but very healthful and lively. Immediately after the birth, I introduced my hand into the uterus, where I found one side of it burst so wide, as to have admitted my hand to pass through the opening." Mr. Haden relates a case that terminated with safety to both mother and child.*

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* Jour. de Med. for 1768.

† Heath's Translations, vol. iii. p. 430.

‡ Med. and Phys. Jour. for March, 1808.

§ System of Mid. § 43, p. 110.

We have said we would challenge the advocates of Dr. Denman's opinion, to furnish a single instance of recovery, from a rupture of the uterus or vagina, at full time, when the fœtus had been permitted to remain undelivered; and we do this without the fear of its being accepted. We are well aware that histories purporting to be of this kind, are before the public; but there is strong reason to call their identity in question. We believe these to have been cases of extra uterine fœtuses, where recovery has been said to follow; we believe this, because, in cases of this kind, at the end of nine months, pains resembling those of labour have come on, and continued for a certain period—they would then disappear, not again, perhaps, to return. But in no one instance in cases of this kind that we have yet examined, have the same symptoms which mark the rupture of the uterus occurred, and it would be easy to cite those where death has eventually ensued, in which it is expressly stated the uterus was found sound. Now, would this be the case, had the uterus suffered a laceration? would not the accident quickly have declared itself? and should we not have found traces on the uterus when examined after death?

We are the more firmly persuaded of the truth of our position, by a case of this kind which recently occurred in this city. In this case, at the usual termination of pregnancy pains were experienced, but the patient was proclaimed by her accoucheur, to be neither in labour nor pregnant—fifteen years after this she died, in consequence of an abdominal abscess—upon opening the body, a fully developed fœtus was found enclosed in a membranous sac—the uterus perfectly sound, and without blemish.[*] I was favoured with a sight of this fœtus and uterus, through the politeness of Dr. Harlan, and refrain from saying more on the subject, as I hope to see a detailed account from the gentleman who attended her.

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* Med. and Chirurg. Trans. vol. ii. p. 113.

This case we feel to be strictly in point; first, because pains like those of labour were experienced, which after a period ceased, and which might be considered by those who espouse the contrary opinion to ours, as a case terminating by the rupture of the uterus, and from which the patient might be said to have recovered. Secondly, Because dissection clearly proved that these symptoms may occur without the uterus being immediately interested, as it was found entirely sound. Thirdly, Because we are of opinion that this case completely explains the supposed recoveries from rupture, where the child was permitted to remain within the abdomen.

We are aware that there is a case recorded in the Journal de Medecine,[†] as a case of ruptured uterus, from which the woman recovered, though the fœtus was suffered to remain in the abdomen. But this case is not strictly in point, and is to be admitted with great suspicion, as an instance of a child escaping into the cavity of the abdomen, after a lesion of the uterus. First, Because the woman was but four months advanced in her pregnancy, and consequently her case does not infringe upon our general position. Secondly, Because, as it is recorded as a case of ruptured uterus, it is more than probable that it had some agency in the woman's death four months after the accident, although not thus stated; the body was opened to ascertain the cause of death; as the woman menstruated regularly after the accident which caused the rupture, great doubt was entertained of her pregnancy. Thirdly, There is strong ground of belief that the fœtus did not entirely escape into the cavity of the abdomen, although it is expressly stated that "a fœtus was found in its cavity;" but this may mean no more than, that upon opening the abdomen, a fœtus was discovered within its parietes, without intending to declare it was not enveloped in a membrane or sac, and which membrane or sac was the peritonæal coat of the uterus,* through which it had not passed. It is not stated that it was found lying loose among the intestines, which we should expect to have been declared, had it been the fact; but merely that it was found within the abdominal cavity. Now we believe this to be strictly true, but does this prove it was not arrested by the peritonæum? we sincerely believe it was.

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[*] See also Dr. M'Knight's case, Mem. Med. Soc. Lond. vol. iv. p. 342, &c. &c.

[†] Jour. de Med. for 1780.

First, Because we can readily imagine that the substance of the uterus may yield, without disturbing the integrity of its peritonæal coat, and the more especially at this early period of gestation, as it is loosely spread upon it, that it may not suffer undue distension in the advanced periods. Secondly, This supposition is strengthened by the appearance of the uterus itself; for it is said that "a rupture was discoverable in the uterus, which was closed and cicatrized on the internal surface, but still open in that towards the abdomen." Now we believe that the wound was prevented from closing towards the belly by the presence of the fœtus, which being in contact with it, acted like an extraneous body, and thus prevented its healing.

These suggestions, we think, are corroborated by the symptoms which immediately followed the accident; namely, "an uterine hæmorrhage." This discharge proceeded most probably, from the lesion of the uterus itself—after a time "the flooding ceased, and the menses appeared;" that is, the discharge of blood from the wounded uterus ceased, so soon as the wound was "cicatrized on the internal surface," and thus restored to its former integrity, "as the menses" (an evidence of its healthy condition within) "appeared at the end of a month."

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* We have, in support of this opinion, Dr. Ross's third case, Annals of Medicine, vol. iii. p. 306. "On opening the abdomen of this cadavre," says Dr. Ross, "it was found under the ligamentum latum of that side, (the left) an arm of the child could be felt, covered only by the peritonæum." Here then is demonstration that the substance of the uterus can be torn without doing violence to its peritonæal covering.

No mention is made of coagula of blood within the cavity of the abdomen, nor of any other extravasated fluid; it is probable then that none existed. But would this state of things have obtained, had the rent passed through both the uterus and the peritonæum? we think not—for, as there was an external discharge, it is more than likely there would have been internal extravasation. But to put this matter almost beyond question, we will relate two cases where there can scarcely be a doubt that the uterus may be so ruptured, as not to produce the solution of continuity of its peritonæal covering; the first is thus related by Dr. Bell.* "Elenor Noon, aged twenty-one years, after being married fifteen months, had the usual signs of pregnancy. At the expiration of her reckoning, was seized with labour pains, which by her own account, were very violent for several days, and continued near three weeks, gradually becoming more languid, until they entirely ceased; after this, she perceived no motion in the àbdomen. But as her pains abated, her belly increased in size, and continued to do so until I saw her," "about five months after she was seized with labour pains. At this time the whole abdomen was so much distended, that I imagined she and her friends had mistaken the case, and that it had been ascites from the beginning." "She was emaciated to the greatest degree, and so weak as not to be able to stand." "I judged it impossible that she could outlive a few weeks." "But upon going to the same part of the country about four months after, I found her still living. I was then informed that, about a month after I last saw her, all the water had been discharged from a small rent at the navel, and had come away in such quantities and such violence, that near four gallons were received in a short time. Mixed with this water, were some fleshy strings, and as the oozing continued, some small bones came away." In consultation, it was thought proper to dilate the opening; this was accordingly done to the extent of four inches, and from the wound were extracted the bones of two fœtuses. "My patient bore the operation better than could have been expected. No hæmorrhage ensued, and she recovered her health so very speedily, as to be able to menstruate in little more than three months. "She became pregnant two months after, went her full time, and had an easy and natural labour, and a healthful child. Since that she has had six more, and enjoys as great a share of health and strength of constitution as most people."

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* Med. Comm. Amer. edit. vol. i. p. 303.

"During the operation I introduced my hand several times into the cavity of the abdomen, both that I might extricate all the bones and other extraneous matters, and that I might be able to judge whether the fœtuses were in the uterus, or any cyst formed for them." "Most of the gentlemen were of the former opinion; indeed, the thickness and firmness of the substance in which the fœtuses were contained, seemed to corroborate that opinion. But besides this, the woman still persists in asserting, that the waters came from her in that labour in the natural way, and in the same manner that they have done in several other labours since. This assertion, if true, must put it beyond all doubt that the fœtuses were lodged in the uterus, and not in any particular cyst." We do not, by any means, agree with Dr. Bell in his last conclusion, as we believe there never has been an instance of a fœtus being retained within the proper cavity of the uterus, beyond a short period after the evacuation of the waters. We, therefore, think it was not so in this case: first, because a tedious and elaborate process was instituted for the removal of the fœtuses, when a much more simple and natural one would have succeeded better, namely, a renewal of the contractions of the uterus. Secondly, That the ulcerative process which nature established, could only have answered the end, by the destruction of a large portion of the substance of the uterus itself. Thirdly, Because we are expressly told she menstruated at "the end of three months," and that "she had a natural and easy labour," and became the mother of six more children. Now is it rational to suppose these things could have happened, had the uterus suffered so severely as it must have done, if the conclusion of Dr. Bell be admitted? We are of opinion this case admits of a much more simple explanation—namely, that the substance of the uterus alone suffered the lesion, while the fœtuses pushed its peritonæal covering before them, and were returned by it, and prevented from escaping into the cavity of the abdomen—the uterus healed after a time, and a sac was formed by the peritonæum, in which the fœtuses were lodged; and this became "thick" from inflammation, and the adhesions consequent upon it.

The other case we intend to relate, is by Dr. John Sims,* a physician of London, of great respectability. "A well formed woman, mother of several children, seven months gone with child, after taking a very long walk, under great agitation of mind, was, upon her return home, seized with an uterine hæmorrhage which continued some days, and then gradually abated, and did not afterwards return; but the woman continued very weak and ailing for two months, when, according to her reckoning, she had completed the full period of gestation. On the 11th of May, 1792, she was taken with labour pains, and sent for her midwife, who gave her expectations of a speedy delivery. But the pains going off, she left her in the evening, with assurances that the child presented right, and that every thing was in a safe way. Next morning, finding herself very ill, but without labour pains, the patient sent for an experienced practitioner in the neighbourhood, who attended, and upon examination, found the mouth of the womb not sufficiently dilated to admit the finger; he could feel no membranes distended with water, nor any part of a child, either through the mouth of the womb, or through the parietes of the womb itself. Her face was bloated, her legs and thighs œdematous, and her belly very large. From these circumstances, he very rationally suspected that she was not with child, and directed his attention to the hydropic symptoms. But as she grew daily worse, I was desired to see her on the 16th of May, when I found her unable to lie down in bed, complaining of violent pains in her side; her respiration was short and frequent; her pulse extremely rapid, with some hardness in the stroke; a fetid black discharge flowed from the vagina; her legs and thighs were much swollen, and pitted upon pressure; the mouth of the womb was relaxed and a little open at the first entrance, just as it is frequently found when unimpregnated in women who have had several children; no part of a child could be felt through the parietes of the womb, nor could the enlargement of this organ be perceived. I was immediately convinced that, if the midwife had given a true account of the case at the time she was first called, a rupture of the uterus had taken place, and the child had escaped into the cavity of the abdomen. With this idea, I examined the state of the belly externally, which was very large and hard to the feel; the tumour circumscribed as in pregnancy, but nothing like the extremities of a child could be felt through the ligaments, and when afterwards the woman was able to lie down and turn in her bed, I could not find, upon examining in different positions, *that the tumour fell to the depending side.*"

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* Med. Facts, vol. viii. p. 150.

"Upon the 18th she was better—the discharge from the vagina was increased and very fetid—pulse one hundred and twenty strokes in a minute—she took tonics with advantage—in a few days more, some of the nails, and a little of the hair of a full grown fœtus (as was judged by the size of the nails) were discharged from the vagina, and these followed by some small finger bones. She continued pretty much in the same state, till about the middle of June. By the first of July she was much recovered—the size of her belly was much diminished—the appetite good—and the discharge had entirely ceased. In short, the poor woman seemed now to be in a state of convalescence, when unfortunately she was persuaded by some foolish adviser, that a good jumbling in a coach would bring on her long expected labour—a coach was procured, and, although the motion of it gave her excruciating pain, particularly about the navel, yet fully persuaded that it was from the shaking that she was to expect a salutary effect, she bore it with fortitude. The pain continued increasing after her return home, with great soreness over the whole abdomen, and she expired early in the morning on the 7th July, two days after the fatal ride."

"When the crucial incision was made through the integuments of the abdomen, the lower flaps could not be turned down, on account of the adhesion of the peritonæal lining with the contiguous parts. But upon carefully dissecting them away, there was immediately discovered nearly the whole of the bones of a fœtus, which appeared to be of the ordinary size of a child born at the full time, or perhaps somewhat smaller." "This mass of bones was surrounded by *a membranous sac* of a black colour, which was entirely smooth on its internal surface, and adhered externally to all the contiguous parts. By these means a complete cavity was formed to contain the bones, *which had no communication with the abdomen.*" "In the anterior part of the cervix uteri next the bladder, was a rent *through its substance*, about three quarters of an inch in length, the sides of which were nearly contiguous, but ulcerated and not disposed to heal. Several small bones of the fingers and toes had fallen down from the mass above into the most depending part of the cavity, and were found lying about this opening. The uterus, very little larger than it is usually found in its unimpregnated state, was in its natural situation in the pelvis, and the fallopian tubes and ovaries were without any morbid appearance. The vagina was perfectly entire, so that there was no passage for the dissolved putrid matter of the fœtus, and the small bones that were discharged, but through this rent in the uterus, and thence through the mouth of the womb into the vagina."

This case we think perfect in all its parts, to prove that the substance of the uterus may be lacerated independently of its peritonæal covering; for it is expressly stated, that the rent was through its substance, and that there was *no communication with the abdominal cavity,* circumstances whic could not have obtained had the peritonæum been ruptured, with the substance of the uterus itself.

But Dr. Sims draws a very different conclusion, and gives this case as one which "*shows the possibility of an escape, even when the child has been suffered to remain in the cavity of the abdomen.*" Does it prove this, either by the detail of the facts or the event? certainly not; on the contrary, it appears to us completely to support our position, that in all the cases recorded of recoveries from ruptured uteri, there was no direct communication with the abdominal cavity through the medium of the laceration, and consequently, cannot militate against our opinion, that there never has been an instance of success, when the uterus has been ruptured at or even near the full period of gestation, and where the fœtus had escaped into the belly of the mother, and been suffered to remain there.

Dr. Sims calls this a case, where the fœtus had escaped into the cavity of the abdomen, though he expressly tells us that the sac containing its bones, "had no communication with the abdomen"—how then can the fœtus be said to have been in that cavity? That the sac containing it had one of its faces looking into the abdomen, we are willing to admit; but this does not place the fœtus there—there was an interposing substance between it and the cavity agreeably to his own statement, and that is all there is, when the child is contained in the uterus; and when there, it might as fairly be said to be within the cavity of the abdomen; or with equal propriety be said, that the lungs occupy a part of it, because only the diaphragm is between them. Would Dr. Sims declare the urine to be within the cavity of the abdomen, because the bladder occupies a portion of it?

Dr. Sims seems to be aware there was difficulty in his way respecting this sac, and asks, "Was this sac entirely adventitious, and formed by a process of nature? or could it consist of the membranes naturally investing the child, retaining, by means of adhesion to the contiguous parts, enough of the principle of life to enable them to withstand the putrefactive process. The uniform black appearance of it may seem to countenance the last opinion; and, although it seems gular in her periods after the hæmorrhage ceased. She was taken to the Hotel Dieu the June following. She had had a violent vomiting for some time, which never ceased during the remainder of her life."

"When the body was opened, there was found in the abdomen a collection of a brown fœtid matter. The omentum was dissolved, and all the parts in the neighbourhood of the liver had suffered by inflammation and suppuration. Adhesions were formed throughout them. The uterus was in a natural state as regards colour, size, and consistence; *but a rent was observed on its back part, which had never united.* In the midst of the adhesions the remains of a putrid child were found."*

In this case the woman was seven months advanced in her pregnancy when the accident happened. She immediately suffered violent pain and had a flooding. She languished for about five months (for the time is not strictly defined.) At the end of this time she died. Some time before her death she was seized with a vomiting, which continued until she expired. Upon opening the body "a collection of brown matter" was found. The "omentum was dissolved," and all the parts in the neighbourhood of the liver had undergone inflammation and suppuration, and had contracted adhesions with each other. The remains of a putrid child was found amidst the adhesions. The uterus, as to colour, size, and consistence, was in a natural state; but a "rent was observed on the back part, which had never healed." Here we at once perceive the effects of an extravasated fluid within the abdomen. From the moment of the accident there was a train of untoward symptoms, which could readily be traced to their source. Inflammation, with all its unpleasant consequences, was the result of a quantity of blood, &c. thrown upon the intestines, and ultimately death.

This is but too frequently the end of this accident; it is confessedly one always of extreme danger, though not uniformly fatal. But we are not in possession, as far as our information extends, of a single fact, where violent and alarming symptoms have not followed from the sudden presence of any extraneous body whatever within the abdomen. It would seem indeed that the powers of the system are sometimes capable of overcoming the consequences of certain fluids thrown into the peritoneal cavity; hence we find that blood, and even the liquor amnii, have not always proved fatal, when they have been discharged into it. But is there a solitary instance of a solid body, like that of the fœtus, having been suddenly immersed in the intestines without the most fatal result following? We think not.

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* Journ. de Med. 1780.

In the case just related, the conservative power of the system was immediately called into requisition; but it was unavailing. In no case, perhaps, was this power more beautifully and conspicuously manifested than in the case in question. It had, by enveloping the fœtus in the folds of the adhesions, greatly diminished the irritation arising from its pressure and motion, and succeeded in obviating the mischief arising from its presence in the abdomen, as far as it was possible to succeed under like circumstances. We think then that this case decidedly proves, by its consequences, that, in the former, the fœtus had never penetrated the peritonæum.

To illustrate our position, we selected one of the most favourable cases* we could find upon record for the adverse opinion; yet it was found that the system yielded ultimately to the disturbance produced by a fœtus of small size, and which we have great reason to believe never had penetrated the peritonæum. If then the powers of the system, after struggling for some months for mastery, be incompetent to the repair of the mischief occasioned by the presence of a small fœtus, how much less able would it be to overcome the consequences, when that body shall be of much larger size? Dr. Sims's case proves the same thing, p. 16.

It may be urged, that in cases of extra-uterine fœtuses, the peritonæum does not suffer from the presence of these

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* The one from Jour. de Med.

bodies, as they are sometimes retained very many
This we are not disposed just now to dispute; but b
the cases there is not the slightest analogy. In the c
an extra-uterine conception, the ovum is so gradually
loped, and of course the peritonæum is so slowly, ind
may say, so imperceptibly distended, that it wou
amount to irritation; and consequently the system
not for a long time be roused to notice its encroach
and may therefore, as already admitted, be carried
years without any serious inconvenience. We have a
analogy to support this explanation, in the formation
abdominal dropsy. The fluid constituting it is so s
and gradually deposited within this cavity, that the
næum is not urged by its effects to notice its prese
quietly, and for a long time submits to be greatly d
ed, without manifesting that it is annoyed by it.

That it is owing to the very slow progress of the
on the one hand, and the very gradual deposition o
on the other, that the peritonæum does not feel much
venienced by their presence, will be admitted, we tru
every unprejudiced mind; and we believe that it will
ceded to by an equal number, that, if a fœtus of a very
size, or the one twentieth part of the fluid which ma
stitute a dropsy, were suddenly thrown into the abd
cavity, that the most serious and alarming symptoms
immediately ensue.

The peritonæum is never, we believe, lacerated
human subject, with entire impunity; and the more
cially where the wound will admit the external air i
abdominal cavity. We must therefore necessarily
the rupture of either the uterus or vagina as an acci
extreme danger. This we are fully prepared to adm
this concession does not amount to the abandonment
hope of recovery from these accidents; for we are ful
suaded that there is no mistake in the cases related
respectable authorities we have quoted. And we are f
persuaded that we should have had more instances o
veries upon record, had these cases of ruptured ute

judiciously treated, or had they always been under the control of those who would have acted with that promptitude the exigency required. For there is a moment to act which will give at least a prospect, nay a probability of success; if this be lost, every exertion may be unavailing. But as this *proper instant for action* is perhaps not indicated by symptoms, we shall always, we believe, be justified in the attempt to relieve, however unsuccessfully the case may eventuate. In Dr. J. Hamilton's case,* he declares "almost every circumstance was unfavourable," yet the patient recovered; while that of Dr. Garthshore† died after languishing twenty-three days, with a variety of symptoms, "sometimes alarming, at others encouraging."

Danger, in cases of the kind we are considering, is not always commensurate with the apparent extent of the injury. Hence the fatal termination in cases where the injury appeared slight; and complete recovery where hope was almost instantly abandoned. Now, as we cannot, à priori, determine the extent of power in the system, or the degree of injury it may have sustained, we should act as if it were competent to the exigency, by removing every obstacle which might oppose its efforts. It would seem that the peritonæum at times will not bear the slightest injury without the most alarming consequences immediately following; nay, death itself has taken place where the lesion of this membrane has been but slight, and entirely confined to itself. At other times, it would appear to suffer, without much inconvenience, extensive lacerations. In proof of the former assertion, we will briefly relate a case given by Mr. C. M. Clarke, in the Medical and Chirurgical Transactions, vol. iii. p. 290. "A woman between the age of twenty and thirty, fell into labour with her first child, about eight o'clock in the morning, under the care of a midwife. Her labour pains came on gradually; but about ten o'clock she was suddenly seized with great pain in the belly and nausea, which did not end in vomiting. Great irritability succeeded, with faintness and excessive restlessness; and at half an hour past ten she died undelivered." "On the following day the cavity of the abdomen was inspected." "The uterus was in some measure contracted." "I gently turned the fundus of the uterus over the pubes with my hand, so as to bring into view its posterior surface. In the fold of the peritonæum which dips down into the pelvis between the uterus and the rectum, I observed above an ounce of blood; and upon that part of the peritonæum which covers the posterior surface of the uterus, there were between forty and fifty transverse lacerations, none of which were in depth above the twentieth of an inch, and many were merely fissures in the membrane itself. They varied very much in length, some measuring one inch, some two inches, whilst the length of others did not exceed the fourth part of an inch. The space upon which they were situated extended from one side of the uterus to the other, and occupied the greater part of its whole posterior surface. The edges of these lacerations were thinly covered with flakes of coagulated blood, and there could be no doubt that the blood found in the peritonæum had escaped from these lacerations. The muscular part of the uterus was perfectly whole." In this case the appearances on dissection could scarcely be called sufficient to account for the death of the patient, yet in every other respect the body was declared sound.

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* MS. Lectures.

† Douglass's Essay, p. 29.

In support of our latter position, we shall relate the history of "Elspet Grant,* who, in April 1736, took her labour pains. After they had continued three days, with the child in the birth, two cracks, as if the rafters had broke, were heard about the sick wife, and her belly was rent from near the navel, with a squaint downwards and to the left side, to near the share-bone. At this rent the child came into the world, the after-birth was brought away, and the entrails were seen."

"The rent was cured without any other application than that of butter mixed with white sugar." This case is certified upon the oaths of a number of witnesses of the fact. These cases we think prove that the event alone is to determine the extent of the injury, and they hold out to us a strong inducement to act in all cases as if success were certainly to crown the exertion.

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* Phys. Essays, vol. ii. 338.

Mr. Burns* has reiterated the sentiments of Dr. Hunter, Dr. Denman, and some others; and as his work has had considerable circulation in this country, the pernicious maxim of "doing nothing" has consequently become more extended. He says,† "to leave the case to Nature, like an extra-uterine pregnancy, is most likely to be successful." We must again ask the question, Has Mr. Burns ever known a case terminate favourably that was thus treated? We repeat the question for the sake of information, as well as to insinuate our full persuasion that this has never happened. If Mr. Burns was in possession of any facts of this kind, it was his duty to have made them public, either in detail or by reference, neither of which has he done. His assumption of the position just quoted, like those of Dr. Denman, is entirely gratuitous; and highly calculated to do injury to the best interests of humanity, and to the improvement of the profession.

From all then we can learn, either from the experience of others, or our own, we are forced to conclude, there is no solid reason for withholding succour from the languishing patient, though our aid may be again and again unavailing. For what can we promise ourselves by a contrary conduct? Since, as we sincerely believe, it is not within the experience of any man, "that a woman has recovered from a rupture of the uterus when the child at or near the full term of gestation, has been suffered to remain within the cavity of the abdomen." We have endeavoured to prove that certain cases purporting to be of this kind, are not entitled to this distinction; and consequently, must not be cited as such. In no department of science, are false facts so mischievous as in the medical; the perpetuation of one important error, may have its thousand victims. And we are not less sincere in our declaration than honest in our intention, when we affirm, there is none of higher grade than that "a patient who has suffered a rupture of the uterus, has a better chance of recovery, by resigning her case to the natural efforts of the constitution, than by any operation or interposition of art."

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* Principles of Midwifery, p. 158. † Id. p. 178.

But Dr. Denman is at variance with himself, for he expressly tells us, "besides some few others, of which I have been informed, or which are recorded, a case has occurred to my very *worthy, able and experienced friend, Dr. Andrew Douglass*, in which the uterus was ruptured, he turned the child, the patient recovered, and had afterwards children."* "If no other case," continues the doctor, "had ever occurred, I apprehend this would be sufficient authority, to render it in future the indispensable duty of every practitioner to act in a similar manner; and bad as the chance is of the patient, to be strenuous in using all the means which art dictates, to extricate her, if possible, from her danger, or to preserve the child."

We may ask what has changed Dr. Denman's sentiments on this head? As for ourselves we are totally at a loss to imagine, though he gives a case in the beginning of his essay which he declares was the cause of it—but we see nothing in this case which should have had this effect; for it is a case of common features, and from which no deduction in our opinion can be drawn either favourable or unfavourable to the subject in question. If the declaration of Dr. Denman was strictly true, that, "there are more instances upon record of recoveries of women, who have not been delivered, than of those who have been delivered after rupture of the uterus," it would merit a most serious consideration; but as we are convinced ourselves, and have endeavoured to prove that this is not the case, the doctor's arguments have not in the least changed our opinion. But let us suppose that this is really the case, should it influence us to withhold our assistance from the poor woman when the argument is not fairly stated? For we must insist, that a vastly greater number of women have been suffered to remain without an attempt to deliver them after the child has passed into the cavity of the abdomen, than have been delivered; consequently the proportions not being equal, the inference must be incorrect.

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* Introduction to Midwifery, vol. ii. p. 117.

Besides, Dr. Denman[*] attempts in a most disingenuous manner to throw suspicion upon the case of his "*worthy, able, and experienced friend Dr. Andrew Douglass*," by declaring there was "no instance within" his "knowledge, except one, *which was doubtful*, of either of them (mother or child) being preserved." Why the case of Dr. Douglass should be doubtful in the year 1810, when it was recorded as an unequivocal instance of recovery in 1795, is extremely difficult to tell—it looks too much like a subterfuge, to avoid the force, which the fact brings with it.

Lamotte[†] who had not the advantage of the experience of others like Dr. Denman to direct him, thought much more correctly upon the subject; and in the only two cases which occurred in his practice, he delivered, and solaced himself with the reflexion, "quelqu' inutile que fût cet accouchement, nous fumes plus contens tous deux, elle d'etre accouchée, parce qu'elle en mourut plus tranquilement, et moi de l'avoir executé." His experience had not furnished him with a successful case, nor had the practice of any one else. He considered the case as hopeless, and was only prompted by the feelings of humanity to give the aid he did; and we see he was satisfied with himself for it.

If we may add our own experience on this subject, it would bear testimony in favour of delivery, where even no question but that of humanity could arise; for we are convinced that we diminish by it, both bodily pain, and that extreme anguish of mind, with which the patient is harassed. Sufferings that need not excite surprise, when we take a view of her unhappy situation; an extraneous substance of great bulk, solidity and weight, to say nothing of the fluids discharged with it, is thrown suddenly upon the naked and exquisitely tender intestines, disturbing their order, interrupting their functions, and inevitably stimulating them to inflammation and all its terrible consequences. To this we may add, in many instances, for a time, at least, the struggles of the fœtus itself.

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[*] Essay on the Rupture of the Uterus, p. 7.

[†] Traite des Accouch. p. 464.

As a moral question, it must we think be decided against the advocates for "doing nothing;" for, there can be no doubt that in many cases, the woman's life is protracted by delivery, and consequently we have no right to abridge it, by the plea that "we are unwilling to give additional pain, where there is already but too much suffering." But if it be decided (and of this we entertain not the smallest doubt) that the woman's life is lengthened by delivery, we shorten it, if we withhold this benefit; and murder itself might in man instances be divested of its criminality if the plea should be admitted, that its victim could have lived but a little time more. It is not for us to limit the sanative power of the system by theorising upon it; it is our duty to give every facility to it, by the removal, as far as may be in our power, of every obstacle that may oppose its progress towards a cure. In doing this we but *obey* the dictates of "*reason and humanity;*" and should our efforts prove successful, however seldom, they would be amply rewarded. Facts alone should direct us upon this subject; and they, in our opinion, emphatically declare in favour of delivery.

It would be highly interesting, were it strictly practicable, to run a parallel between the cases where delivery was effected, and those where the woman was suffered to die without it. We are persuaded we should generally find the woman to have lived longer, and in some instances much longer for it; this alone, if a fact, would claim a serious consideration. If we are to judge from the instances furnished us by the records of medicine, (and we have not at present any other mode by which we can determine,) and take for granted that the delivery was the cause of the protraction of death, we should without the smallest hesitation decide upon it as the preferable mode of practice; but as these cases may be urged as mere coincidences, we shall not insist upon them as proofs, as we wish this matter to be settled upon as solid a foundation as the nature of the case will admit. We shall therefore not consider the deductions from them as absolutely conclusive, though highly probable, and strongly tending to corroborate our general position. In estimating the period of death from the moment of the rupture, at least as certainly as that period can be determined, it will be found that in almost every instance, the time from the accident to that of death is longest where delivery was resorted to. We regret we cannot at this moment have a more extensive range for comparison, but as no case shall be concealed that we are in possession of, we trust the inference will be fair from the data, if not entirely convincing. We have bestowed considerable labour to collect facts upon this subject, and we feel that we have estimated their force without partiality.

We have examined nearly all the cases collected by Dr. Douglass by referring to the authorities, and find them faithfully related; we shall make use of these, with others furnished by our own reading, for the purpose just proposed. We shall first give those cases where delivery had taken place; and then will follow, those where the women died without being delivered.

	Lived after delivery
Lamotte's first case,[a]	3 days
second case,[b]	4
Garthshore's,[c]	23
Peu's,[d]	8
Steidele's,[e]	7
Dewees's[f]	5 hours
James's,[g]	a few minutes
Douglass's,[h]	17 hours

[a] Observ. 312.

[b] Observ. 313.

[c] Douglass, p. 35.

[d] Pract. des Accouch. p. 341.

[e] Observ. de Rup. Uter.

[f] Coxe's Med. Mus. vol. ii.

[g] Med. Repos. vol. i. p. 325.

[h] Essay, p. 40.

Cases undelivered, with their periods.

Monroe's case,[i]	2 days after rupture
Garthshore's, (as quoted by Douglass, p. 35 of Essay)	2 days after rupture
Pouteau's,[k]	36 hours probably*
Saviard's,[l]	but a few hours probably
Denman's,[m]	16 hours probably
Smellie's,[n]	14 hours
Hooper's,[o]	12 hours
Chatard's[p]	4 days, or as many hours; there is great uncertainty in this case; most probable but a few hours.

From the foregoing statement of an equal number of cases of each kind, taken precisely as they occurred in the search, we find that those women who were delivered, lived much longer, on the average, than those who were not delivered; and though we are willing to admit this is not strictly proof, yet we must think it is rendered highly probable that the delivery of the child suspended the moment of death. If then it is highly probable that the life of the patient is prolonged by our efforts, it becomes an indispensable duty to make them; but if we add to this, that there are many well attested cases of recovery where the child has been extracted, and not one to prove the opposite position, there cannot, or rather should not, be a moment's hesitation about the plan to be pursued.

2.—The causes which may tend to a rupture of the uterus are much multiplied by authors. Some, in our opinion, are wholly inadequate to the end; while others of powerful agency are slightly passed over. We shall not pretend to enumerate all that are assigned, but will notice some of the most prominent.

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*I have put probably to several, as the hours are not exactly mentioned.

[i] Essays Phys. and Lit. vol. i. p. 339.

[k] Œuvres, p. 487.

[l] Observ xxv. p. 131.

[m] Douglass's Essay, p. 40.

[n] Midwifery, vol. iii. p. 385.

[o] Mem. Med. Soc. of Lond. vol. ii. 118.

[p] Med. Repos. vol. vi. p. 128.

It has been supposed by many, that the force exerted by the child against the parietes of the uterus might occasion its rupture. Lamotte, we believe, was the first who suggested this cause; and considering the few opportunities he had to observe this complaint, his opinion is both natural and respectable. He says,[*] "Lorsque l'accouchement s'est déclaré par de legères douleurs, qui sont devenues trés violentes, les membranes qui contiennent les eaux s'ouvrent, et l'enfant y joint ses efforts, étant dans une bone situation, et ne se trouvant point d'obstacle qui empêche sa sortie, c'est une chose bientot finie: mais si au contraire quelque chose se trouve qui l'arête au passage, comme une tête trop grosse, et les os ilion, ischion, et pubis, par trop serrez, c'est une necessité que les violens efforts que cet enfant fait, réflechissent contre le fond de la matrice, qui ne se trouvant pas toujours d'une égale consistance, ni assez forte pour résister si longtems aux impétueuses saillies de l'enfant, ses parois sont à la fin obligez de céder et de se rompre." Of this opinion was also Levret, Crantz, and some late writers; but we are perfectly of the opinion of Baudelocque, that "the child is almost always passive at the time the uterus tears. If it becomes the instrument of the rent, it acts no otherwise than any other solid body of the same volume, inanimate and of an angular surface would, on which the uterus should be strongly contracted." We may add, in confirmation of this suggestion, that the uterus has been ruptured after the death of the child.

Dr. Denman says,[†] "independently of disease, the uterus may be worn through *mechanically*, in long and severe labours, by pressure and attrition between the head of the child and the projecting bones in a distorted pelvis, especially if they be drawn into points or a sharp edge." Can it be imagined that the cause here assigned can act in the manner Dr. D. supposes? Can the friction or "attrition" spoken of exist to the extent necessary to produce the effect? We think not: for before the membranes are ruptured the head of the child cannot be so firmly pressed against any resisting point as to produce this "attrition;" for the instant it comes in contact with it, it will recede, in consequence of the liberty it enjoys amidst its waters; and after the liquor amnii is evacuated, the uterus embraces the fœtus so firmly in every point, that the mechanical action called "attrition" cannot take place. Besides the smooth surface of the uterus and the soft one of the child's head might pass over each other successively a thousand times without wearing the uterus through. Again, action and reaction being equal, the scalp of the child should be abraded as well as the uterus. Now this condition of the child's head has never, so far as we have heard, been noticed.

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[*] Traite des Accouchemens, p. 462.

[†] Introduction to Midwifery, p. [illegible]

Burns says,* that "in most cases it is owing to external violence." He must, we think, mean, where this accident takes place before the full term of utero-gestation, and not during the period of labour. In this, perhaps, he may be right; but if we compare the whole number of cases which medical writings furnish us with, we shall find that those which happen during labour are much more numerous than the others, and have not "external violence" for their cause. He indeed does not appear to be aware that he has given this opinion, for in page 267 he says, that "the most frequent cause, however, of this accident is a disproportion between the size of the head and the capacity of the pelvis, by which a portion of the cervix uteri is pinched between the head and the pelvis, and fixed so that the action of the uterus is directed against this spot rather than the os uteri." This explanation is totally unintelligible to us. We cannot see why the powers of the uterus should be more forcibly directed towards that portion of itself that "is pinched between the head and the pelvis," or if they were, why this should occasion it to tear sooner than it would the os uteri, when directed against it, unless an important change in the condition of the part thus impinged upon had taken place; for until this change happens (of which we shall speak presently) this part is as strong, ceteris paribus, as any portion of the uterus; and when it does take place, it gives way, not because "the action of the uterus is directed against this spot rather than the os tincæ," but because it has become the weakest part, and cannot now sustain the common efforts of the other portions of itself.

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* Principles of Midwifery, p. 159.

Mr. Burns says,* that "Salmathus considers a thinness† of the uterus as a predisposing cause of rupture; and Dr. Ross‡ relates a case where it *seemed* to have this effect, the womb not being above the eighth part of an inch in thickness, and tearing like paper." We cannot well determine on what principle Salmathus supposes a "thinness" of the uterus to be "a predisposing cause of rupture," since we have no evidence of this state existing before the rupture has taken place, but as an accidental or temporary condition; and then, perhaps, it is rather an imagined than a real occurrence. Certain it is, we have no way to ascertain the fact previously to the accident it is said to be the cause of; and a post mortem examination will not permit us with certainty, to declare that this "thinness" existed as an original structure. That the uterus is found thin upon dissection, in some instances of rupture, we cannot doubt; but that that was the state of the uterus before the rent, and existing as an original conformation, we have in no case a proof; nor does Dr. Ross's case, as quoted by Mr. Burns, countenance any such belief. Dr. Ross does not give it as a cause of the accident which killed his patient; he merely mentions the fact without either comment or conjecture. That the uterus was unusually thin and tender we do not pretend to dispute; but we certainly would deny, without further evidence, that either or both of these circumstances were the cause of the rupture. Its "thinness" was owing, in our opinion, to a cause by no means uncommon in these cases, namely, an excessive hæmorrhagy, which so effectually emptied the uterus of blood, that it could no longer maintain its accustomed thickness at this period; for there is no question now how the uterus supports this thickness; it is universally admitted to be from the augmented capacity of its vessels. It therefore follows as a consequence, that when exhausted of blood by the emptying of these vessels, its thickness must be diminished in proportion to the reduction of their calibers,* provided the uterus has lost its power of contraction, or is mechanically distended by any agents within its cavity. Its tenderness was owing to a pretty advanced stage of putrefaction; for Dr. Ross says that the uterus was "emphysematous," and "was exceedingly distended, and of an uniform figure, as if blown up." And this condition favoured the further distension of the uterus, and rendered it, most probably, much thinner than at the moment of the rupture.

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* Principles of Midwifery, p. 258.

† By "thinness" we presume is to be understood a departure from the natural and ordinary thickness of this organ, as an original conformation of it; for if it were an accidental or temporary condition, there must be some cause to produce it; and that cause operate in such manner, as to stretch the parietes of the uterus beyond their usual degree of tenuity. In this case the "thinness" is but the effect of a preceding cause, and this "state," we suppose, must always exist where there is a powerful distracting cause, or in other words, that the uterus always becomes thin before it is stretched to bursting; but that it does not always burst by being stretched.

Annals of Medicine, vol iii. p. 277.

"Mental agitation" and "frights" are said to produce the rupture of the uterus. Of this we entertain strong doubts. When these do act upon the uterine system, it is, most probably, only to excite labour prematurely. This, it is true, may be followed by a rupture of the uterus, but we believe nothing more. Dr. Sims's case was one which was preceded by "great agitation of mind;" but we have every reason to believe that the rupture did not take place for two months after the first effects of this "agitation" "and a very long walk" had manifested themselves in the production of "an uterine hæmorrhage."

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* That this is the true explanation of the "thinness" of the uterus is corroborated by a case related by Smellie, vol. iii. p. 361. The woman died of flooding, and he says the "uterus of the woman's body seemed to be quite destitute of blood, for scarce a drop appeared on opening the parts," and the uterus "was not a quarter of an inch thick." We shall also give a case which fell under our own observation tending to confirm the same explanation.

Having noticed in a brief manner a number of circumstances purporting to be causes of the rupture of the uterus, and offered a few remarks upon each, to show our reasons for not admitting them to be such, we shall now proceed to consider those causes which must be allowed as adequate to produce this effect. They may be conveniently divided into two kinds: those which act directly, and those which act indirectly upon the uterus.

The first or direct, are mechanical violences, and may be external or internal. The external may be a blow,[*] a fall,[†] a kick or violent pressure;[‡] the internal may be, attempts to turn,[§] or to return a prolapsed limb,[||] or the mal-adroit application of instruments,[¶] or the unequal surface the fœtus itself may present.[**]

The second, or indirect are those which impair the integrity of the substance of the uterus, such as all those causes which offer a mechanical impediment to the passage of the child, as a contracted pelvis[††], an unusual sharpness of the linea iliopectinea,[‡‡] and exostoses,[§§] tumours,[||||] scirrhous indurations,[¶¶] and ulcers.[***]

The first set of causes act directly by exerting a force beyond the resisting power of the uterus itself; the second by diminishing the strength of some particular portion of that viscus, so that its own contractile powers are sufficient to overcome the resistance which the injured part offers to them when strongly excited.

The operation of the first set of causes is sufficiently obvious without any explanation; and the influence of the second is by far the most common, though not so immediately evident. When there is a contracted pelvis, the head of the child is prevented from freely engaging in it; and as it is always covered by the uterus at the commencement of labour; and in most instances until the last period; it follows as a consequence, that it must be confined between the presenting part, and some portion of the pelvis; now, if it be too long retained in this situation, and more especially if it rest against a sharp edge, as the linea iliopectinea,* or an exostosis,† inflammation will ensue, and, if the cause be not removed quickly run on to gangrene. When this has taken place, it is easily understood how rupture may take place at the part thus injured, even without any extraordinary exertion from the healthy portions of the uterus.

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[*] Journ. de Med. 1780.

[†] Hamilton's MS. Lectures.

[‡] Journ. de Med. 1780.

[§] Dease as quoted by Burns.

[||] Annals of Med. p. 278.

[¶] Dr. Hunter, Med. Journ. vol. viii. p. 368, as quoted by Burns, p. 481.

[**] See Mrs. M.'s case of this essay.

[††] Perfect's cases. p. 448.

[‡‡] Burns's Prin. Mid. p. 268.

[§§] Med. Mus. vol. 2.

[||||] Baudelocque, vol. iii p. 413.

[¶¶] Perfect's Cases, vol. iii. p. 439.

[***] Baudelocque, vol. iii. p. 413.

The second set act, by preventing a regular distension of the uterus, during gestation; of consequence, some one portion or other is unduly put upon the stretch, and of course weakened—and by being passive during labour, from its diseased condition, cannot resist the efforts of the healthy portions.

Baudelocque‡ says, "the violent and sometimes convulsive action of the uterus on the child's body, is almost always the cause of its rupture." But that "the rupture in question cannot happen in any case, unless the uterus has been predisposed to it by those means (those we have just enumerated) or by other causes which are all accidental." It follows then, that if every point of surface of the uterus, is equally disposed to support the efforts made to expel the child, the rupture cannot take place; but if any one part is weakened from any cause whatever, that portion necessarily runs the risk of laceration. When the action of the uterus itself is the remote cause of rupture, the latter always happens at the moment when the former is exerting its highest power; hence this takes place in the height of a pain. This accident may happen to any portion of the uterus or at its connexion with the vagina; and the rent or tear may be in almost any direction. It may be more or less extensive, so that the child with its appurtenances may escape entirely into the cavity of the abdomen, or only part of it, or it may remain confined to the uterus. These different conditions are not alike unfavourable: when the child leaves the uterus altogether or partially, the woman's chance of recovery is much less, than where it does not pass through the wound, for a mere lesion of the uterus itself is not necessarily fatal. It is the injury done in the abdominal cavity by the presence of a foreign body that chiefly constitutes the danger.

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* Burns, p. 268.

† Dewees's case, Med. Mus. vol. ii.

‡ System, vol. iii. p. 412 and 413.

This accident when it happens is almost always announced by very decided symptoms, which shall now be considered under the third division of our subject.

3.—It has been supposed by some, that the rupture of the uterus was preceded by symptoms which pretty distinctly announced this event to be at hand. Of this opinion was Crantz and Levret, agreeably to Baudelocque; Crantz says,* that "when a woman is threatened with a rupture of the uterus in a laborious labour, the belly is very prominent and tight; the vagina lengthened, and the orifice of the uterus very high; the pains are strong, leave little interval, and do not advance the delivery." Even these latter symptoms, though much less equivocal than those which precede them, are by no means certain; the case of Mrs. M. which we shall soon relate will prove that an extensive rupture may take place where the circumstances of "strong pains, with little interval," was not among its precursors. To these signs M. Levret† adds, "that the pain the woman suffers, is always seated towards the middle of the epigastric region; that a last effort or violent leap succeeds to the repeated strugglings of the child, which announces its death and the rupture of the uterus." Most of the signs just enumerated, belong to almost every case of "laborious labour," especially the prominence and tightness of the belly; these appearances take place almost necessarily from the condition and relation of the parts to each other; for in almost all cases of this kind, the waters are drained off, sometimes earlier and sometimes later; but in proportion to the time, will be, ceteris paribus, the force with which the uterus will employ its tonic power of contraction, and this power accommodates the parietes of the womb, almost strictly to the varied surface which the fœtus presents; this then accounts for the tightness; and the prominency is entirely owing to this same power, bringing the presenting part immediately over the opening of the pelvis, which it is obliged to enter, if it enter at all, at an angle of at least thirty-five degrees, consequently the fundus must be thrown in an equal degree forward, that its axis shall correspond with that of the superior strait; and this arrangement constantly takes place, and the uterus is equally "prominent and tight" in every case of labour where the waters have been drained off for any length of time; yet a rupture is comparatively of rare occurrence; on the other hand, when the waters are not discharged, this prominence and tightness cannot take place, yet there have been instances of rupture while the membranes have remained entire.* As regards the length of the vagina, we know this must vary; it must be long or short as the presenting part may be far or near from the os externum, for when there is a narrowness of pelvis either absolute or relative, or an anterior obliquity of the uterus existing, there the vagina must necessarily be "lengthened," yet rupture is very far from happening whenever either of these circumstances obtain; while on the other hand we know that rupture has taken place when the head of the child has been low in the pelvis,† and consequently the "vagina" very short.

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* Baudelocque, vol. iii. p. 420.

† Ibid.

The sign which Levret has added to those indicated by Crantz, is equally doubtful; for instances of rupture have occurred where there was no pain in "the middle of the epigastric region," and many times none has followed this symptom; and it is a fact long since upon record that the uterus has yielded, after the death of the child.‡ We therefore perfectly coincide with Baudelocque, "that the rupture of the uterus has often taken place without being preceded by any of them, and has not happened in other cases where their union declared it inevitable;"* and most heartily concur in the opinion, that "if we were to take them for our guide, we should sometimes touch upon the rights of nature, by performing a delivery which she would have been able to terminate without inconvenience."†

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* Smellie, vol. iii. p. 385; also Mem. Med. Soc. vol. ii. p. 118.

† Douglass's Essay, p. 50.

‡ Annals of Med. vol. iii. p. 293 and 303.

Dr. Douglass‡ in reference to Mrs. Manning's case, which he has so circumstantially and clearly related, and from which she most fortunately escaped, says, "I shall ever retain the most lively impression of the nature and appearance of the poor woman's throes; of the agonizing sensations they seemed to excite in her, and the little effect they had in propelling the child. These, with a recollection of the alarming accident of the rupture of the uterus which followed, would amount to a degree of internal evidence of what I had to apprehend, which I should not think myself at liberty to reject." And confesses "that was another case to occur, resembling in most of its previous circumstances" that of Mrs. Manning's, he "should be tempted to turn and extract by the feet."

We do not mean to interfere with the conduct of any gentleman under the apprehension of rupture of the uterus; as we presume under so trying a circumstance, every one would act most scrupulously in the way he thought best; nor should we condemn any man for an error in judgment; yet we should be very far from recommending as a rule of practice the one suggested by Dr. Douglass, where there was a strong combination of symptoms which might seem to announce this event at hand; for who by a forced delivery, can flatter himself that he has prevented the uterus from lacerating? Could moral certainty be arrived at by the attending symptoms, our profession would gain much; we could then prevent the rupture, and most probably secure the life of both mother and child; but as this certainty can-

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* System of Mid. vol. iii. p. 421.

† Ibid Loc. cit.

‡ Essay, p. 47.

nanded, and as threatening cases are very com-
ı, and the accident comparatively very rare, we cannot
:l justified in recommending artificial delivery on the
.ere presumption of a contingent advantage. We would
e the more cautious on this head from the consideration,
hat, wherever this might be thought expedient from the
mptoms, there would also be a risk of producing the ac-
.dent, the operation purported to avoid; the waters long
rained off, the uterus contracting strongly, and firmly em-
racing the body of the child; the latter very large, or the
elvis contracted, offer difficulties which the inexperienced
an neither appreciate nor anticipate, and the experienced
would dread to encounter; and should the practitioner dare
he enterprize, and, by perseverance on his side, and reso-
ution and suffering on the part of the patient, achieve the
lelivery, can he solace himself, or honestly assure the woman that he has saved her a ruptured uterus?

We will now relate the case we have hinted at above, as it contains several peculiarities; the patient was originally under the care of my friend Dr. Samuel Stewart, whose account we shall employ until the period at which we were called upon to see her.

"On the 4th or 5th of July, 1820, I was requested to visit Mrs. M——. She made no particular complaint, but expressed some uneasiness at having passed over a few days (five or six) the time she calculated she should have been confined; a circumstance which had never occurred with her before, although the mother of five children. I assured her those mistakes were very frequent and should cause her no uneasiness."

"On the 14th, the membranes ruptured and the water was evacuated without any previous pain; nor did any supervene till the morning of the 17th, when about ten o'clock I was sent for. The os uteri was very much dilated, and the vertex presented at the brim of the pelvis; the pains not being severe, and recurring at long intervals, I left her, with a request, that as soon as they became more frequent and s
vere, they should immediately send for me. About t

o'clock they sent and stated that her pains were quite severe; fearing lest I should be too late, as some of her former labours had been rapid, I went immediately, but before my arrival the pains had entirely ceased. The os uteri was now fully dilated, and the cervix somewhat lower."

"Expecting every moment a recurrence of the pains, I waited for some time, but finding they did not come on, she was requested to walk about the room; and while walking, the form of the abdominal tumour was so particularly situated, that several females who were present, remarked it; the tumor was less round, and more flat above, and acute in front than usual. This appearance, as I had not the slightest suspicion of the fact, I explained to myself by supposing the great laxity of the abdominal muscles had allowed the fundus of the uterus to fall more than usually forward, and the length of time since the waters were discharged had given the uterus full time to contract over some projecting part."

"Near an hour had now passed without any return of pain, and the pulse being in a state nearly natural, it appeared a fair case for the use of the secale cornutum; ten grains of it were given, and finding it to produce no sensible effect, after an interval of half an hour, ten grains more were exhibited; although she felt no regular pain, she complained of excessive uneasiness and distress, and repeatedly declared 'her child would kill her.' "

"Between seven and eight o'clock, still suspecting no danger, I again determined to try the ergot; this was given in the same quantity, at the same interval, and with the same inefficacy as before. It was now after nine o'clock; she felt herself becoming worse—her uneasiness was excessive, and her spirits almost exhausted, yet her voice was strong—her pulse was feeble and sinking—and now, for the first time, her case appeared alarming, and I began to fear something terrible had taken place, and Dr. Dewees was immediately sent for, but by the time he came, she was nearly or altogether pulseless, yet still retained her recollection, and considerable strength of voice."

When I arrived, the patient was in the situation stated by Dr. Stewart; I immediately conjectured the nature of the accident, and it was agreed that no other chance remained but from instant delivery. Dr. Stewart sat down for this purpose, and was soon in possession of the feet. At this moment Dr. James arrived, who had also been sent for with myself. He concurred in the measure adopted, and kindly waited the event. After the body of the child was delivered, great difficulty was experienced in bringing down the head. Dr. S. was considerably fatigued by his exertions, and I took his place: upon passing up my hand to adjust the position of the head, I found it of an unusual size, and that every attempt to deliver it whole would be in vain—it was agreed it should be opened, and Dr. S. went in search of the proper instruments; but before he returned, life had so far ebbed, that any other attempt would be useless—the body was separated from the head—and the poor sufferer quickly after breathed her last.

Leave was obtained next day to open the body—accordingly Drs. Chapman, Stewart, Horner and myself went for the purpose of the examination, and Dr. Horner proceeded to the operation. On opening the abdomen, a large tumour of a globular form presented itself, which was the uterus, inclosed in which, was the severed head—a stratum of coagulum covered the superior part of the uterus—the uterus was thin and flaccid—in the abdomen was about three pints of extravasated fluid blood. Upon raising the uterus and carrying it towards the symphysis pubis, a laceration was discovered, running from the vagina to the right superior part near to the insertion of the fallopian tube—the rent was entirely through the neck and body of the uterus, but did not extend into the fundus. The placenta maintained its connexion with the uterus. The vessels of the large intestines were much injected—the peritonæum on the anterior of the abdomen was studded with red vessels—the bladder was sound—the lower part of the wound where the rent began. was gangrenous.

The head of the fœtus was removed from the pelvis, i which, from its size, it could not engage, and was subsequently subjected to examination by Dr. Horner, who has favoured me with the following results.

" The horizontal circumference of the superior part of the cranium measured . . . . 1 foot 10 inches.
" Diagonal circumferance . . . . 1 11½ do.
" The cerebellum, of natural size and condition.
" Cerebrum devoid of convolutions, ¼ inch thick.
" Lateral ventricles containing three pints of water.
" Bones somewhat larger than usual, and the sutures widely separated."

This case is remarkable, first, in the entire absence of symptoms which would lead to the suspicion, that a rupture was about to take place. Second, in the perfect freedom from those marks which distinguish this accident after it has taken place; for there was neither vomiting nor even sickness—no fainting nor disposition to it—no frequency of pulse, nor hurried respiration. Thirdly, no particular event or expression decided the moment at which the uterus gave way—no exclamation from sudden and acute pain, nor any noise to characterize the injury—no external hæmorrhage—in a word, nothing to lead to the suspicion that a laceration had happened. Fourthly, that in this case, there was neither "deformity of pelvis," "exostosis," nor unusual "sharpness of the linia iliopectonea"—no "tumour," "scirrhous induration," nor "cartilaginous condition of the os uteri"—"no external violence," nor "internal force" to account for it. But the dissection renders it probable that the uterus was predisposed to the accident before the period of labour; and it clearly demonstrated, that it was owing to the pressure of the child's head against the projection of the sacrum, inducing gangrene in that portion of the uterus that was included between the points of contact. But, strictly speaking, this might be considered as a case of relative deformity of the pelvis, as the enormous size of the head rendered the opening of the superior strait too small for its passage, so that all the effects that would arise from an absolutely diseased one was produced.

maintain its globular form, we have a right to conclude that either our suspicions are not exactly confirmed, or that the fœtus is still retained within the cavity of the uterus, though the rupture has taken place. But this is not to lull us into a security that may be fatal to both mother and child.

If the accident happen after the rupture of the membranes, the presenting part will either recede beyond the reach of the finger, or will be so easily forced back, (provided it has not absolutely engaged in the pelvis) as immediately to excite alarm, if not confirm suspicion. Under such circumstances we should not trust to the "touch" alone; the hand should be cautiously introduced into the vagina, and the most careful and deliberate examination be made. This examination will detect, not only the rupture, but the part that has sustained the injury; should it be the uterus itself, we shall be able, with little or no force, to pass the hand through the os uteri, if the accident has happened after the labour had been well advanced; and this would lead to the knowledge of the exact situation of the patient. But should the uterus have given way before the os tincæ was sufficiently dilated to pass the hand freely, we should not be tempted to use a force that might be as destructive as the accident we were dreading; we may, however, profit by the situation of the hand, as it will enable us to pass the finger far, if necessary, into the uterus through its mouth, and by this we can ascertain whether the fœtus still entirely occupies it, for in this case it will be within a very small distance of the os uteri; if it has partially escaped into the abdomen, it will most probably be more remote; and if it be entirely in that cavity, it will no longer be within reach of the finger. We must obey the same rules should the rupture take place before the full period of utero-gestation.

But if the laceration happen to the neck of the uterus, or at its connexion with the vagina, it is much more frequent that the fœtus with the placenta pass immediately into the abdomen; in either of these cases, the presenting part will suddenly and entirely remove from the superior strait; we are immediately to examine the patient, so soon as these symptoms render it more than probable that this event has taken place. There does not exist the same difficulty to a satisfactory examination in these latter cases as in the former; for when the uterus is lacerated at its neck, the wound for the most part is so extensive as to permit the hand to pass without difficulty into the abdominal cavity; as the orifice of the of the uterus is prevented from contracting by this rupture of its circular fibres; and when the rent takes place at the union, if we may so term it, of uterus and vagina, there is, perhaps, even less difficulty to the passage of the hand; for the wound cannot diminish in capacity in any direction by the contraction of any of its fibres, but would rather augment, if they possess such a power; the uterus will be found for the most part firmly contracted, either on the anterior or posterior portion of the pelvis, as it may happen to be either the anterior or posterior portion of the vaginal circle that has given way.

The intestines will frequently prolapse through the wound, which but too decidedly declares the nature of the accident; or we may encounter them immediately above the edges of the rent; and no one that has not experienced this trial, can possibly imagine the thrill of horror with which he will be seized, the instant his hand comes in contact with the naked bowels. It is almost needless to suggest the propriety of a cautious and gentle examination, when the hand has entered the abdomen. The difficulties and danger which must necessarily accompany a laceration of the uterus, may be augmented by a portion of intestine being engaged in the wound, and there severely pinched by the contracting orifice.

4. Having noticed, as briefly as the importance of the subject would admit, the three first divisions of our subject, we shall now proceed to the fourth and last; in which we propose to consider "the mode of proceeding under all the various circumstances with which this accident may be complicated." From what we have said in the commencement of this paper, it will be perceived, that we have no reliance on the powers of nature to effect a cure when the child has

scaped into the abdominal cavity; and that we are of opinion, that nothing but the prompt and judicious interference of art, can rescue the unfortunate woman from the impending fate with which a rupture of the uterus threatens her.

For the interference of art to be even *probably* successful, it must not be delayed a moment beyond the detection of the accident, whenever it is practicable to seize that moment; for we have no hesitation to believe that there would have been much fewer victims from this cause, had the practitioner been aware of the nature of the case, or sufficiently intrepid to have instantly acted, when he had ascertained the uterus was ruptured. The injury which the system sustains from this casualty, is not simply from the lesion of the uterus itself; but also from the additional evils which must follow from an inflamed peritonæum. This would seem inevitable from the very nature of the accident, and must consequently be augmented in proportion to the continuance of the exciting cause. To be useful then, we cannot be too early in the removal of the offending bodies from the cavity of the abdomen, and by this means abstract a powerful and never ceasing stimulus; for we must repeat our conviction, that there never has been a recovery, where the fœtus, &c. were allowed to remain.

Believing then, we have said enough to convince an unprejudiced mind that the positions and conclusions of Dr. Denman, if not gratuitous, are certainly erroneous, we shall proceed to consider the methods which should be pursued under the various combinations in which they may present themselves, and may be arranged as follows:—

I. When the laceration is confined to the body or fundus of the uterus, but penetrates the peritonæum, and the child escaped into the abdomen; this may happen,

- *a.* where the pelvis is well formed.
- *b.* where the pelvis is deformed.
- *c.* where the uterus contracts firmly.
- *d.* where the uterus remains flaccid.*

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* This case does not seem clearly recognized by Baudelocque, and makes hi somewhat at variance with himself; for in § 2175, he says, "for notwithstandi

It may happen, 1. where *a.* and *c.* are combined.
2. where *a.* and *d.* are combined.
3. where *b.* and *c.* are combined.
4. where *b.* and *d.* are combined.

II. Where the laceration may pass only to the peritonæal coat, and the child not into the abdomen; this may happen,
a. where the pelvis is well formed.
b. where the pelvis is deformed.
c. where the uterus contracts firmly.
d. where the uterus remains flaccid.

It may happen, 1. where *a.* and *c.* are combined.
2. where *a.* and *d.* are combined.
3. where *b.* and *c.* are combined.
4. where *b.* and *d.* are combined.

III. Where the laceration is confined to the neck of the uterus and vagina, and the child, &c. has escaped into the belly; this may happen, *a.* where the pelvis is well formed.
b. where it is deformed.
c. where the uterus contracts firmly.
d. where the uterus remains flaccid.

It may happen, 1. where *a.* and *c.* are combined.
2. where *a.* and *d.* are combined.
3. where *b.* and *c.* are combined.
4. where *b.* and *d.* are combined.

that accident," (the rupture of the uterus) " it is not always impossible to extract the child by the usual passage. De La Motte, and others, furnish examples of it, which I do not quote to serve as models. The former turned a child, searching for the feet through the rent in the uterus, as far as the middle of the belly whither they had penetrated; and others assure us they had brought back a child that way, which had entirely escaped out of the uterus;" but adds immediately after, " which will appear not very probable to those who know how much the uterus contracts from the moment it is emptied, and how much the rupture then loses of its extent." Though the uterus does, for the most part close, as stated by M. B. yet we know from a number of cases which are upon record, and from our own experience, that this is not invariably the case; and, as far as our observations extend, the uterus is wont to remain *flaccid*, if the rupture be accompanied by a considerable discharge of blood. Besides, the case quoted from La Motte, on which he seems to place reliance, was an instance in which the child had passed entirely into the abdomen. For La Motte in his " Reflexion" on this case, says, that the body was opened after death, and that " l'on ne trouva à la marice que le vestige de cette ouverture, dans laquelle l'on ne put introduire que le bout du petit doigt, quoique le corps de l'enfant y eût passé tout entier." La Motte Obs. cccxvii. See also Douglass's Essay, No. vii. p. 29, No. viii. p. 31.

the mind peculiarly their own; nor should we be able to overcome the appalling sensations they produce, if we were not influenced by paramount considerations. To save life is a strong motive to the operation; and to be snatched from death is a powerful inducement to submit to it. Where this is the only resource, the case should be fairly and candidly stated, that no after blame may attach; and in all cases of such hazard, responsibility should be divided, by requesting the concurrence of a brother practitioner, where time too precious would not be lost in this compliance. It has been called a "horrible expedient" by Dr. Douglass.* It is so confessedly; so are lithotomy and many other operations; but this is not to be the test. Its utility alone ought to determine whether it should be considered as a resource of our art, or be for ever proscribed. For we are by no means satisfied with Dr. Douglass's reasoning upon this subject. He asks "if a rupture of the uterus is of itself an injury so generally fatal, what is the patient likely to gain by combining the dangers of such an accident with those of a penetrating wound which will expose the abdominal viscera?" Dr. D. appears to have forgotten that there was already "a penetrating wound" which "exposed" the abdominal viscera; and that an additional one through the teguments would scarcely enhance the risk, since we know that wounds of this kind are not necessarily mortal.‡

Besides, what would Dr. D. have us do in those cases where there is no possible alternative (as VII. *a.* or in I. 6.) but this operation? for it is only in cases similar to these that the operation is recommended. The woman can but die after the operation; and this she certainly will do if it be not had recourse to. And what practitioner would not prefer an alternative that may succeed though hazardous, to the abandonment of a patient to the unrestrained consequences of disease?

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* Essay, p. 51.

† Ib.

‡ We find three highly interesting cases of the extirpation of the ovaria, in which there was a speedy restoration to health, although the wounds through the teguments of the abdomen were extensive, and its cavity a long time exposed to the air. In neither of these cases did any untoward symptom arise, though in the first case the tumour was so large as to contain fifteen pounds "of a dirty glutinous looking substance," and the sack which contained it, after being extirpated, "weighed seven pounds and one half." In the second, notwithstanding every care was taken to prevent it, a quart of blood was spread among the intestines, yet no unpleasant symptoms are said to have arisen. In the third, a diseased ovarium was taken out, which weighed six pounds, yet the patient recovered "in two weeks." Dr. M'Dowell's cases, Eclectic Rep. vol. vii p. 242.

That it has been successfully employed we are not at liberty to doubt; nor is there any testimony that it has been either wantonly employed, or that it has added new sufferings or new dangers to the already almost certainly fatal disease, for which it is proposed as a remedy. We shall add the evidence we are in possession of, that it has been successfully performed, and from it allow every one to draw his own conclusions as to its advantages. As regards ourselves, we have no hesitation in believing that it is exclusively indicated in several combinations of ruptured uterus. Mons. Thibaut des Bois, a surgeon of Mans, has given an account of this operation having been successfully performed on a woman several hours after the accident, though too late to benefit the child. He adds, "that the woman suffered scarcely more than from the consequences of a common labour."* M. Laussus† quotes a history of this operation having been twice performed with entire success on the same woman; and as the case is highly interesting, and the work from which it is taken is not much known in this country, we have translated it for the satisfaction of our readers.

"A woman of a strong constitution, of about thirty years of age, and pregnant for the fourth time, was seized with strong labour-pains. One pain was so particularly severe as to occasion her to faint. Immediately after there was a discharge of blood from the vulva; the hand was introduced into the uterus, but the child was not found within it. The pains ceased, the faintings became more frequent, the extremities cold, and the pulse agitated. Convinced that the uterus was ruptured, and that delivery could not be effected f a child, at full term, we must open the abdomen—but if e deviation be not so great as to prevent this, we may atempt delivery by the first mode.

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* Journal de Med. for 1768.

† Pathologie Chirurgicale, par M. Lassus, tom. ii. p. 237.

II.

In cases such as II. where *a.* and *c.* are combined, as at 1. or where the pelvis is well formed, but the uterus contracted, the only chance is by gastrotomy, and then cutting through the peritonæal coat of the uterus, so as to free the child from it. But where II. and *a.* and *d.* are combined, as at 4. or where the uterus remains flaccid, we must attempt the delivery by the first mode. And where II. *b.* and *d.* are combined, as at 4. or where the pelvis is deformed, we must proceed as directed, for I. *b. a. d.*

III.

In cases such as III. where *a.* and *c.* combine, or where the pelvis is well formed, but the uterus, as far as it is concerned, contracts firmly, we should employ the first mode, unless the rent in the vagina be so large as to permit without much greater tearing, the child to pass through the pelvis. In the latter instance, we believe there would be less risk in a moderate extension of the vaginal wound, than from gastrotomy. But where III. *a.* and *d.* are combined, as at 4. or where the pelvis is well formed, but the uterus is inert, we can with great expectation of success, attempt delivery per vias naturales.

In cases III. where *b.* and *c.* are united, as at 3. or where the pelvis is deformed, and the uterus contracting, it is scarcely to be expected we can succeed by the first mode, although the pelvis may not be very faulty; we may, however, attempt it by very gentle means first, but be sure to desist, should a great deal of force be deemed necessary; in the latter case, we can only expect to relieve by the second mode of operating. But where III. and *b.* and *d.* are existing together, it is possible to deliver per vias naturales, the deviation in the pelvis be not great; but should it be co siderable, our only chance is by the second mode.

IV.

In cases like IV. where *a.* and *c.* are combined, as at 1. or where the pelvis is well formed, and where the uterus contracts, it will immediately occur that the latter cannot, either in this case, or where the uterus remains flaccid, oppose any difficulty to delivery according to the first mode; so that in these two cases, we cannot believe that gastrotomy can be justifiably performed. But were IV. and *b.* and *c.* or *b.* and *d.* are combined, or where the pelvis is faulty, and the uterus is either contracted or flaccid, it may be indispensable, provided, the deviation in the pelvis be such as would prevent a child at full term to pass, unless the child be very small.*

V.

The management of the cases V. where I. and III. are complicated with a descent of intestine, is precisely the same as regards the rupture of the uterus, as directed for these cases, together with a strict attention that the protruded or strangulated gut may be reduced. This is, perhaps, one of the most formidable cases that can present itself to the already but too much perplexed practitioner; for he has not only to contend with a ruptured uterus, but has a strangulated intestine to add to its horrors. We are but too well aware that the latter alone, if not quickly relieved, is sufficient to destroy the patient; and the practioner may, after flattering himself he has by the delivery of the child and placenta, given his patient a chance for life, have the mortification to see her expire from this cause, which he only detects after her death. Where this complication takes place, with a contracting uterus, whether the pelvis be well or ill formed, there is no possible chance but in gastrotomy—nor are we certain that even this will give a chance of recovery; for how shall we be led to suspect that a portion of intestine is included in the contracted wound? Does not this difficulty suggest the propriety and authorize the act, to pass the hand freely and carefully round the uterus; with a view to discover whether any part of the gut may be found detained in the lacerated portion of it? For, from merely reasoning on the subject, it would appear very probable that it might be readily detected, as it would prevent the hand from passing uninterruptedly over the surface of the hardened womb. But should it be discovered, how is it to be relieved? That it would be difficult, we readily admit; but, that it is not beyond the resources of a well instructed accoucheur, we are as ready to believe. An incision cautiously made so as to enlarge the opening, would certainly free the intestine from its confinement; but whether the part, by which it is held in durance, would always be sufficiently in view to ensure a successful operation, may reasonably be doubted; though there is a strong probability that this combination would only happen upon either the anterior portion of the uterus, or upon its sides. Now in either of these situations, we should conclude, à priori, that it would be possible to liberate the gut.

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* Baudeloeque says, § 2178, "the section of the coverings of the abdomen will not appear so indispensable after a rupture of the vagina, as after that of the uterus," but without specifying the cases in which it might be necessary. He appears to have forgotten what he had just before advanced, that the Cæsarean section (and consequently gastrotomy, when the uterus is ruptured) is exclusively indicated where the pelvis is absolutely too narrow; and adds, "it is much less the fear of a rupture of the uterus, which leads us to perform it, than the impossibility of terminating the delivery in any other way" Is this then not deciding that gastrotomy should be performed in cases of ruptured vagina, when the pelvis is "absolutely too narrow?" Baudelocque, vol. iii. § 2173, p. 424.

If the intestine descend through the opening in the uterus, and the latter remain flaccid, and the pelvis well formed, it may be returned after the delivery of the child. But if it take place under similar circumstances with a pelvis too small to deliver per vias naturales, it may remain down, after the operation of gastrotomy has been performed for some time, without betraying its situation; nor, indeed, until the returning powers of the uterus should include it within the edges of the wound, and thus strangulate it; in this case, the patient may have had the appearance of doing well, and perhaps was doing well; and possibly might have recovered, but for this new accident, for which no remedy could be offered, as we could not know its existence, nor remedy it if we did.

r we presume no one would have the hardihood to open
e abdomen a second time, upon the bare suspicion that a
'angulated gut was the cause of the existing untoward
mptoms.

Baudelocque* mentions a case that was communicated to
e Academy of Surgery of Paris by a country surgeon,
io declared he had relieved a portion of strangulated gut,
carrying his hand, armed with a bistory, into the uterus,
d enlarging the ring formed by the contracted wound, and
is, three days after delivery. There is considerable diffi-
lty with us, to reconcile this story with the concomitant
cts; for the uterus is stated to have so much contracted
e dimensions of the wound, as to infringe upon the pro-
ıded gut. Now if the uterus was so reduced as to stran-
late a portion of intestine, how was it practicable to carry
e hand within it, and find sufficient room there for this
splay of surgical hardihood and adroitness? we confess our-
lves at a loss to imagine it.

VI.

In cases such as VI. where *a.* and 1. are combined, or
iere the head is at the superior strait of a well formed
lvis, we should not hesitate to turn or deliver by the forceps,
ovided the attending symptoms would justify the belief
at the uterus was ruptured, and the os uteri sufficiently
lated to admit of either of these operations. We should
efer the former mode, when the waters had been but re-
ntly drained off, or when the uterus was flaccid, the lat-
r when the contrary obtained. Where *b.* and 1. existed,
where the pelvis was well formed, but where the head
is engaged in the superior strait, the forceps are exclusive-
indicated, unless we are sure of the death of the child,
en the crotchet may be used. And where *c.* and 3. are
iited, or where in a well formed pelvis, the head is low,
th a firmly contracting uterus, the forceps must terminate
e delivery, if the child be living—the crotchet, if dead.

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* Midwifery, vol. iii. p. 433.

If *c.* and 4. unite, or where the head is low, the uterus iner and especially if the child has in great part escaped from i the forceps alone must be thought of, unless the child certainly dead; in this case, the crotchet. If any other pa than the head present, the case must be treated as if th particular case required immediate manual assistance; f instance, should the breech present and not rapidly advanc we should bring down the feet and deliver, the same if th knees; and should the feet, or but one present, we shoul deliver as quickly as would be judicious under any oth circumstance where it was proper for art to interfere. Whe VI. and either *a. b.* or *c.* are united with 2. 3. or 4. th same rules will apply as when this accident happened i a well formed pelvis. If any other part besides the hea present in a pelvis but moderately defective, we must con duct the labour with strict regard to the woman's safety an the child's welfare; if it be dead, we may expedite the de livery without regard to the child.

VII.

In cases like VII. where *a.* is combined with either *b.* *c.* or where the pelvis is much deformed, so as to preclu the possibility of a child at full term to pass alive; an whether the uterus be contracted or flaccid, there is but o alternative either for mother or child, and that is the Cæs rean section. We are aware that the crotchet may be r commended in these cases, but with what prospect of su cess? none certainly to the child, for that must necessaril fall a victim; and what is the chance for the mother? W believe none whatever—for the force that would be requ site to open the head, and the extreme difficulty to extra it, if that could be accomplished, would exhaust the woma and she would most probably expire before the delive were effected.

With respect to the mode of performing these operation we have nothing either new or material to offer; we mus therefore, refer to Baudelocque and others for the details o this subject. As regards the after treatment, it must be co ducted as if we were treating for peritoneal inflammation, under its various states and conditions of the system, with the addition of a wound in the external teguments; as it is through the medium of this wound, in many cases, we expect the extravasated blood, &c. to issue, we should take care that it should not be closed too soon or too strictly.

Art. VII. *Cases illustrating the Use of the Tourniquet in Palsy.* By S. Calhoun, one of the Physicians to the Pennsylvania Hospital.

THE restoration of the power of the muscles debilitated by long continued inactivity, is an important indication in the treatment of palsy. The application of the tourniquet to the extremities, to effect this end, is the subject of this communication.

A patient was admitted into the Pennsylvania Hospital with a paralysis of the extensor muscles of the leg. He had slept for four hours in a sitting posture with one extremity crossed over the other, and awoke with the usual symptoms produced by pressure upon the sciatic nerve; the limb was partially insensible and incapable of motion and of supporting the weight of the body on attempting to stand. After two hours, these symptoms disappeared from the thigh and leg, the extensor muscles of the foot, however, continued paralytic; in walking the foot was held at right angles to the leg, appearing as a dead weight appended to the extremity, and communicating a halting character to the motions of the whole side.

The power of motion in the extensors of the foot was completely lost; the skin anteriorly for two inches round the ankle joint was insensible to the prick of a pin or to pressure by the finger; the sensation of heat remained perfect.

The part diseased was evidently the fibular nerve, for the extensor digitorum longus, the extensor pollicis, the tibialis anticus, and the skin anteriorly on the ankle, on which this nerve is distributed, were the paralytic parts. The superficial situation of this nerve and its consequent exposur confirmed this opinion.

That the cause of the disease was pressure is rendere probable by analogy in other cases. Sudden pressure upo the nerves in the armpit has produced palsy in luxations o the humerus downwards, and paralysis of the extenso muscles has been caused by the too tight application of b dages and splints in the treatment of fractures of the for arm.

The conjecture that the quantity of blood in the paralytic parts was deficient, induced the application of the tourniquet, as an increase of circulation had been observed t follow its removal.

The pressure was made for half an hour, so as to stop th pulsation of the artery upon the top of the foot, and repeated four times a day. The perspiration of the foot increase during its application, and a glow was felt upon its remova The sensibility gradually returned to the skin over the ankle, the power of motion succeeded, and in about ten week the patient was perfectly well.

Another case of the same kind has since occurred. I four days after the application of the tourniquet the diseas began to abate; the patient, however, was discharged fro the hospital before the cure was completed.

Pressure upon the limb acts by suspending its function for a short time, and thus rendering it more sensible to th operation of the blood, and the influence of the brain. Th effect of pressure upon the limb generally, as also upon th nerves, the arteries, and the veins separately, will now b considered, to analyse more completely the mode of opera tion gradually became extinct, at the end of twenty minutes, when the experiment terminated. The temperature of the surrounding air was eighty degrees.

In another experiment upon the arm, the power of motion first ceased, loss of sensibility succeeded, and the temperature of the hand fell from ninety-seven to ninety degrees in fourteen minutes. These experiments were repeated with the same results; and show that diminution of sensation, of motion, and of temperature, are the result of pressure upon the whole limb.

On removing the tourniquet, increased sensibility was evinced by a pricking sensation, similar to that which succeeds the removal of pressure from the sciatic nerve, a sense of fullness, a sudden glow, and, in most cases, a speedy restoration of the power of motion.

The loss of sensation, of the power of motion, and of temperature will now be referred more exactly to their causes, by examining successively the effect of obstructing the functions of the nerves, the arteries, and the veins.

First.—The nerves. In the right limb of an animal, every nerve was divided, but the blood-vessels were left entire. After three minutes no motion could be excited in the limb,* proving clearly that the nerves, independently of the blood-vessels, furnished the powers of sensation and of motion.

The observations of Earle show that the nerves are intimately connected with the production of animal temperature, for in palsied parts, where the functions of the nerves were injured, but the circulation continued free, the heat of the limb varied with that of surrounding bodies. The limb also had not the power of resisting heat or cold, as in perfect health.

The effect too of stopping the circulation in aneurisms of the lower extremity, as it is often not attended with a diminution of temperature, confirms the opinion that the nerves are much concerned in the production of the heat of the human body.

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* See New England Journal, vol. iv. p. 288.

ond.—The blood-vessels. The effect of the suppressi the circulation in the arteries is to diminish, but not e irely to extinguish the sensibility and the power of motio

The circulation was destroyed by tying up the iliac ar y, and dividing the blood-vessels which supply the ba part of the limb; the nerves were left entire: the limb co tinued to move for one hour, whenever it was irrita though no blood was carried to it.*

The nerves then have a great share in the functions the limb. Sensation, motion, and temperature are in a gr degree the result of their power; the effect of the tourniqu is therefore referred chiefly to these organs; for the obstru tion of the arteries diminishes but does not suspend t powers of motion and sensation.

An experiment upon the obstruction of the circulation i the veins was made by Kaaw Boerhaave. He tied up t vena cava, and produced loss of motion in the posterior e tremities.†

All the powers of the limb then are reduced by pressur Increased excitability to the impression of the blood, an the influence of the brain and nerves is the consequence, a appears from the increased sensation and temperatur when the pressure is removed; thus, by the frequent rep tition of the operation, the palsy is cured.

The application of the tourniquet in this manner is als indicated by the following fact, which shows the connexio of the presence of blood with a state of complete health i those muscles which are perfectly under the influence o the will.

In animals, as the dog and cat, which are not accustome to much exercise, from their habits of domestication, thos muscles which are not frequently exerted become white whilst those which are much and constantly used in the ordinary movements of the animal, are of a red colour, a fac which shows the connexion of the blood with the perfec state of the muscles. In curing diseases it is of importance to restore all the circumstances of health to the morbid parts; of course, as redness of the muscles attends the continued and perfect exercise of their functions, it is worthy of attention; and as the increased flow of blood into the limb after removing the tourniquet will probably produce this increased colour, it becomes an important consideration in fulfilling the indications of cure.

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* See New England Journal, vol. iv. p. 288. Also Hall. Oper. Minor. sur le Mouvement du Sang. 57 & 58. Expt.

† See Richerand's Physiology, p. 310. Phil. Ed. 1808.

As the application of the tourniquet requires considerable accuracy, the effect of the compression of the artery is frequently incomplete from the inattention of the patient. The pressure is made upon the limb generally; the nerves, of course, are affected, and a partial loss of sensation is the consequence; the artery is left open; the veins are more or less compressed, and thus the whole limb is distended with blood; the circulation is increased in parts where it before had been languid, established where it had not before existed, and the effect of the remedy is gained even under a disadvantageous application.

Pressure by the tourniquet has been applied to the limbs of three patients affected with general palsy with good effect. In one instance the disease succeeded rheumatism, and was produced by exposure to rain. In another it was the effect of intemperance, in the third the cause was unknown.

The remedy, however, appears to be peculiarly applicable to local palsy. The ordinary internal remedies are objectionable, because they debilitate the organs of digestion, and endanger the habits of the patient.

Art. VIII. *A Singular case of Tænia, or Tape-Worm, communicated by* Charles Caldwell, M. D. Professor of the Institutes of Medicine, &c. in the University of Transylvania.

Mr. J. P., formerly a respectable merchant of New Orleans, now of Louisville, in the state of Kentucky, had been subject to tænia for three or four years. The affection having been troublesome, obstinate, and debilitating, several physicians had been consulted, and a cure attempted by the use of all the customary remedies. But though some relief had been obtained, nothing had proved sufficient to eradicate completely the offending cause. Spirits of turpentine, rust of iron, and various preparations of mercury, had been exhibited in succession, with but little effect. An article which had proved somewhat more operative and salutary, was tin filings, administered in large doses, and followed by an active saline purgative.

By steady perseverance in these means, thirty feet of the worm had been discharged in the space of about three years. The longest fragment evacuated at one time, measured three feet. The expulsion of this, succeeded the copious employment of tin filings.

At length, in the month of January of the present year, the following prescription, derived most probably from Darwin's Zoonomia, was given by a physician, who, if he had not acquired celebrity for his skill, had at least become noted for the intrepidity of his practice.

℞ Hydrargyri, } *aa* ℥ xii. f. Amalgama.
Stanni,

This metalic paste was directed to be divided into twenty-four doses, each, of course, weighing an ounce; and the doses swallowed at regular intervals of one hour. In the length of his intervals only, did the prescribing physician deviate, in his practice, from that of Dr. Darwin, who orders the article to be taken every *two* hours. The process was to be terminated by a saline pergative.

Although the remedy prescribed, appeared somewhat formidable, yet Mr. P. being a man of firmness, and having sustained from his complaint great inconvenience, and no small degree of distress, determined to pursue the course directed. Accordingly, having procured from Dr. Wilson,* of Louisville, the requisite amount of amalgam, prepared for the occasion, and divided into ounce doses, he commenced the process, and faithfully persevered in it, until he had swallowed fifteen ounces, in the same number of hours.

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* Dr. Wilson was not the prescribing physician.

Experiencing, by this time, a disagreeable sensation of weight in his bowels, he resolved to proceed, for the present, no further. On the following day he took the saline purgative prescribed, which, operating actively, expelled a portion of the worm, measuring forty feet.

Along with this portion, confined chiefly within the folds, was discharged a small quantity of the amalgam. Several minute parcels of it were afterwards evacuated, but the main bulk remained in the bowels. This Mr. P. repeatedly stated to his physician, confidently assuring him that he felt distinctly the metal in his intestines.

Incredulous of the fact, and perhaps considering his patient hypochondriacal, the physician declined an examination, and ceased, at length, to regard the case as an object of attention.

Shortly afterwards Mr. P. was called by business to New Orleans, the state of his health being in no degree meliorated. In the course of his voyage he had the mortification to find, that the whole of the tænia was not yet expelled.

On his return to Louisville, determined to finish the experiment he had begun, he swallowed four doses more of the amalgam, which added to what he had antecedently taken, amounted to twenty ounces. From this second effort, no salutary effect resulted. Saline purgatives were again resorted to without any further discharges of the worm.

Of the whole metallic mass swallowed, Mr. P. does not believe that he has evacuated more than two ounces; consequently the remaining *eighteen* are still in his bowels.

Such, substantially, are the details of the case, communicated to me by Dr. Wilson, as well as by the gentleman who is the subject of disease.

Of the truth of that part of the narrative, which alone appears extraordinary, there is ample testimony. The metallic mass, of a roundish figure, and in an impacted and firmly contracted and resisting condition, is plainly and distinctly perceptible to the touch. Its size is about that of a pound bullet; and its weight, for it can be easily made to rest on the fingers, denotes at least an equal amount of matter, perhaps a little more.

This substance is firmly fixed in the intestine, attached to *one* spot; but the intestine itself is perfectly moveable. Hence the ball may be made to describe around the cavity of the abdomen, a kind of circle, of which the umbilicus constitutes nearly the center. In every part of this circle the ball is perceptible. When, by leaning forward, the patient places his trunk in a horizontal position, it falls near to the umbilicus, and throws its entire weight on the hand when applied beneath it. It can also, when thus situated, be readily grasped between the fingers and the thumb.

On the general health of Mr. P. this large extraneous body, though it has lain nearly eight months in his intestines, has produced as yet no obviously deleterious effect. His appetite and digestion are as good as they have been for many years, and his alvine evacuations are regular and natural; once or twice, of late, he has had slight sensations of colic, but thinks them referable to irregularities in diet.

It must not be inferred, however, from this part of our statement, that the ball is productive of no inconvenience. The reverse is true in an eminent degree. On horseback, our patient cannot travel without real suffering; nor is the motion of a carriage much less troublesome to him: even walking hastily down stairs, stepping incautiously over a gutter, or any other jolting movement proves painful. On a late occasion, the irritation of the ball during a short journey on horseback, produced priapism.

Out of the preceding narrative arise, very naturally, several questions.

1. In what part of the intestinal canal is the amalgam lodged?
2. By what kind of mechanism is it retained in its position?
3. What are likely to be its effects on the system?
4. What mode of treatment is best calculated to afford relief?

5. Does there exist on record, or in tradition, any similar case, to serve as a precedent, or to sanction by authority?

The two first questions being nearly allied, may be considered in conjunction; and it will be readily perceived that the answer to both must be, in a great measure, conjectural.

Without needlessly wasting the reader's time by a detailed exposition of the grounds of my opinion, I shall, on this topic, briefly observe, that it appears to me most probable, that the metallic ball is lodged in one of the cells of the ascending portion of the colon; where it is retained partly by contraction, and in part, perhaps, by the formation of a new membrane, closing the mouth of the cell, and converting it into a cyst.

The production of new membranes and entire sacs, under such circumstances, is a fact well known to physiologists, and must be referred to the agency of the physical power, which watches perpetually over the wants of the system, and labours to supply them. Thus, a leaden bullet, as well as fragments of other metals, lodged in the fle ', become encysted; new sockets are formed for the heads of dislocated bones; joints destroyed are often regenerated when necessary, and in some instances, those already existing, but which have been rendered useless or inconvenient by accident, have been stiffened by anchyloses.

In reply to the third question, it may be observed, that much will depend on the patient himself. The effects of the accident will, most likely, become serious or otherwise, according to the prudence and regularity of his general regimen and course of life. By a life of perfect moderation and care, he may escape disease to a very advanced period, and finally drop into the grave through the outlet of old age. But, from an opposite course, disease and suffering and premature death, would seem inevitably to await him.

The most important topic, the means of relief, is next to be considered.

Under this head much might be said but very little, I apprehend, can be done. Even strong efforts would seem to be imprudent.

By none of the customary modes of evacuating the alimentary canal, can the extraneous substance be dislodged. The experiment, repeatedly and vigorously made, has always failed; nor does any sound principle of practice justify its renewal.

The only expedient left untried, is the use of the knife. But this, although spoken of by many, and by some recommended, will scarcely be resorted to under existing circumstances. The operation being not only painful but hazardous, could be justified by imperative necessity alone.

Were the sufferings of the patient intolerable, or his life placed in immediate jeopardy, by the presence of the amalgam, at every hazard it ought to be removed. But while neither of these exigencies exists, to open the abdomen would be inconsiderate and rash.

Let the patient pursue steadily a well directed course of life—his food simple and easy of digestion—his drink mild and bland—his mind tranquil—his spirits cheerful—his exercise moderate—his clothing suited to the temperature of the weather—his bowels free, and his repose regular and sufficient for the purposes of refreshment and renovation, and he may pass his days in health and comfort and rational enjoyment. He may be exempt not only from suffering, but from all well grounded apprehensions of it; a condition of existence, under which man should be resigned, contented, and grateful.

In relation to the fifth and last topic of enquiry, I shall only remark that, as far as my information in the history of medicine extends, and my memory at present serves me, the foregoing case is without precedent, and, therefore, without the sanction of authority, either as to its probable issue or the mode of treating it.

In the intestines of many domestic animals, some wild animals, and even of man himself, large masses of foreign substances have been repeatedly found. Cases are recorded where, under a long and superabundant use of magnesia or lime, those articles have concreted into solid balls, obstructed the intestinal canal, and ultimately produced death.

In these instances, however, the masses of matter have accumulated gradually, giving time to the surrounding parts to accommodate their condition and susceptibilities to their increase. But I doubt if medical records, traditional or written, furnish a single case where sixteen ounces of metallic matter were swallowed in a few hours, and retained in the intestines without proving fatal.

Should this paper have any effect in enforcing the observance of great caution in the employment of new and powerful remedies, the writer will find no cause to regret the time he has spent in composing it.

ART. IX. *A Case of Lumbar Abscess attended with Artificial Anus in the Groin.* By W. E. HORNER, M. D., Demonstrator of Anatomy in the University of Pennsylvania.

THE acknowledged importance of pathological anatomy, in exhibiting the alterations of structure produced by disease, and in indicating proper modes of treatment, leads me to hope, that the ensuing communication may not be wholly unacceptable to the medical public. The path, however, has been so often explored, and there is such an accumulation of histories and observations handed to us by the labours of Bonetus, Morgagni, Prost, Portal, Baillie, &c., together with the valuable, though less systematic writings of many others, that our eagerness for the perusal of recent cases is very much diminished, and indeed the recorder of them, though impressed with a sense of their utility, is often induced to suppress their publication, from a persuasion that want of novelty is most commonly attended with a want of interest. Nevertheless, as a true miser is insatiable of riches, so one who is actuated by that degree of professional zeal necessary to the improvement and extension of the science, will receive with attention every new communication of facts, will consider every addition to the capital as enhancing the intrinsic value of the stock, and as usefully multiplying references for his own guidance.

I have heard it said by a distinguished surgeon, that the operation for the extraction of the cataract, which is generally supposed to be invariably uniform in its proceedings and different stages, is incessantly modified by little circumstances of which books can take no notice, or else give only a very inadequate idea, though they are of such importance that an inattention to them, or a want of ingenuity in the operator to follow their indications, will produce most mortifying embarrassments, and not unfrequently total failure. The same observation is applicable to almost every other individual case of disease; and hence the more we become acquainted with varieties, the greater certainty we shall attain, as well in the application of remedies as in the detection of such cases as are past the powers of the art.

The case which I am about to relate, though common enough in its kind, still had a modification which distinguished it from others, and which may, with some propriety, be selected for its peculiarities.

On the 15th of August last I was requested by Dr. Gebhard to examine a patient of his who had died in Callowhill near Garden-street, in consequence of psoas abscess. The account of the case afforded me was as follows:

James Culberton, a carter, aged twenty-four years, of a very strong and athletic constitution, in consequence of some injury or strain received in the prosecution of his business in August 1814, felt a severe pain arise in the lumbar and iliac regions, which was followed with fever and the common symptoms of inflammation.

Bleeding, purging, and other means usually resorted to on such occasions were adopted, without any other effect than that of alleviating in some measure the intensity of the attack. From the inadequacy of the remedies to produce discussion, suppuration took place, the matter forming passed out under Poupart's ligament, and produced a prominent and fluctuating tumour, a little below it. This tumour was opened with a lancet, and a considerable quantity of pus discharged. With little subsequent attention the wound got well, and the energies of the patient's constitution seemed to have restored him in a short time to his ordinary state of health. In the middle of the next winter, from being exposed to a snow storm, he caught cold;—the place in the groin which had been occupied by the tumour and in the vicinity of the incision became heated, painful, red, and tumefied;—suppuration followed, and a spontaneous opening was made, through which the pus, mixed with feculent matter, was discharged. Doubt might have been entertained at this time in regard to the real nature of the feculent discharge: but the patient having eaten some rice shortly before, it so happened that a few grains of it passed undigested through the opening. This accident demonstrated that a connexion existed between the intestinal canal and the orifice in the thigh. A number of perplexing considerations grew out of this circumstance, 1st, it was thought that a femoral hernia, lying under the tumour, might have been wounded by the plunge of the lancet; but upon a rigid scrutiny into the case, it appeared, that the patient had never in his life been affected with any of its symptoms, and a local examination of the part itself at the time, indicated nothing of the kind. It, however, was considered possible for it to have had a temporary existence, and to have been afterwards returned by the voluntary action of the bowels, assisted by the position of the patient. 2d. If the primitive inflammation in the lumbar and right iliac regions had invaded the intestinal canal, and by the process of adhesion to it and of ulceration into its cavity, established a communication, it was still a desideratum to ascertain what portion of the bowels was thus affected. In ten days this orifice healed, and continued so till April 1815, a period of three months. At the expiration of this time another inflammation about the orifice took place, followed by suppuration and ulceration; fæces were again discharged for ten days along with the pus; the parts then healed and continued so till February 1817. During this interval the health became sufficiently good to induce him to consider himself exempt from the chance of similar attacks; but these hopes were destroyed about the middle of February, by a recurrence of the inflammation in the thigh, with discharge of fæces.

Between this date and the following August four fresh attacks supervened; after that he suffered from them every three or four weeks till December. It is now necessary to observe, that in the recent recurrences of the disease, the old orifice, which had been situated not far from the anterior superior spinous process of the ilium, remained cicatrized, and a new one formed nearer the pubis. Also that the inflammation which had been the precursor in every instance of the discharge of fæces, was not attended latterly with suppuration, but that the integuments affected by it, being protruded by the accumulation of fæces, burst and gave vent to them. The inflammation then subsided and the part healed. It was therefore apparent that the fæces occasioned the inflammation in the groin. The inflammation had, during this time, extended itself into the contiguous parts, and amongst others, affected the inner side of the thigh, two thirds of the way down to the knee.

The sequel of the case, as communicated to me by Dr. Gebhard, is as follows: In December, 1817, he first saw it. The patient was then labouring under a violent symptomatic fever; the most rigid antiphlogistic plan was pursued, such as bleeding, low diet, refrigerants, &c. By these means he was relieved from the constitutional affection. The subsequent exhibition of bark seemed to strengthen him, and to diminish the intensity of succeeding paroxysms, which took place in connexion with the bursting of fæces through his thigh every two or three weeks till his death.

In February 1820, he became anasarcous and continued so until the middle of July; he was then seized with a sore throat, which in its sensations of pain was extended along the œsophagus into the stomach. The sensibility of those parts became so highly excited, that the swallowing of such bland fluids as flaxseed tea, milk and water, &c. was excruciating. Examination showed the pharynx and palate inflamed, but no swelling of the tonsils; gargles being ineffectually exhibited, the application of a blister on the throat relieved him. The dropsical affection began to disappear simultaneously with the commencement of the sore throat; and by the time the latter was cured, the former had subsided entirely. A diarrhœa had attended the dropsical affection from its beginning—his health sunk apace under its influence, notwithstanding the continuance of a good appetite. The exhibition of medicines suppressed the discharge per anum, but produced no effect on that through the artificial opening. Wasted by this continual drain from the alimentary canal, he at length died on August 15th, 1820.

Examination post mortem. The appearance of the abdominal viscera, generally healthy. The psoas magnus and iliacus internus muscle, which were considered to be the seat of the primary affection, had lost entirely their muscular fibres, were diminished in size, somewhat indurated and converted into a ligamentous-like mass. The head of the colon besides, being bound down to the right iliac region by the usual reflection of peritonæum, had contracted an extensive adhesion to the contiguous muscles. The peritonæum at the part alluded to, seemed unaffected by disease. It is known, that the colon at its head, is covered only in two thirds of its circumference, and that anteriorly, by the peritonæum; the rest of this portion of the gut being in contact with the iliacus internus muscle, and connected to it in health by loose cellular substance; it is the latter part that I allude to, as having united itself to the iliacus muscle, by a preternatural adhesion, which was strong and so compact as to produce a continuity of substance with the muscle itself. At this place, about an inch and a half above the valve of the colon, two orifices existed through the parietes of the gut, each large enough to admit a finger; these orifices communicated with one fistula of the same size, in the centre of the iliacus internus. The fistula passed out of the abdomen, continuing in the centre of the muscle, till it had got beyond Poupart's ligament; it then became superfi-

cial, and terminated in the orifice of the groin, so often a
luded to. A prosecution of the dissection exhibited and
ther fistula arising from the lower termination of the firs
and extending downwards six inches, parallel, or nearly s
with the femoral vessels. It was not known whether this fi
tula had arisen at the commencement of the disease, or fro
the aggravation of the local inflammation which took plac
about August 1817; its connexion with the other was subs
quently demonstrated by the ability of the patient, to discharg
fæces from the external orifice, by pressing along the cour
of the adductor muscles from below, upward. The fem
ral artery and vein were imbedded as far as the place whe
they perforate the tendon of the triceps adductor, in a lig
mentous sheath, which, I presume, had been fabricated
order to protect them from the extension of the diseas
in their vicinity. The parietes of each fistula were so thic
and perfect, that they might have been with ordinary facil
ty dissected from the contiguous parts, and preserved in the
membranous form. The vertebræ or the loins were in
healthy condition.

This case I believe to have been unusual, in regard
the communication established between the abscess, situate
in the iliacus internus and psoas magnus muscle, and the cav
ty of the colon, and it may, perhaps, prove serviceable, b
calling in similar cases the attention of the practitioner
the cause of a series of symptoms, which embarrassed e
ceedingly all the medical gentlemen who were consult
about it. It is also a good example of the species of lu
bar abscess, which, in the language of Mr. Abernethy, pr
ceeds from phlegmonoid inflammation in the part. T
circumstance is familiar to most surgeons, that there a
two species of lumbar abscess; the one preceded by pai
tumefaction, increased heat, throbbing and a hurried circul
tion of blood through the loins, and a secretion of coagul
ting lymph, by which the parts are united to each other, a
the boundaries of the abscess circumscribed, constituting
all these symptoms common inflammation; the other speci
belongs to chronic abscesses, in which collections of matt

take place without any evident act of inflammation. In these latter, the surrounding parts remain in a great degree unaffected by the diseased action; the purulent discharge commences from very small beginnings, increases gradually, accumulates indefinitely, its boundaries are soft and unattended with thickening, affording but little impediment to its gravitation, and the matter, therefore, passes from one part to another, appearing sometimes in the groin, sometimes just above the knee, and occasionally in the perinæum, giving occasion by this change of place, to the distinction which Mr. Hunter has made between abscess *in a part, and of a part.* This last form of the disease, supposed to depend on a scrophulous habit, has its peculiar mode of treatment, which is very satisfactorily illustrated in Mr. Abernethy's Surgical Observations; but the former, differing essentially from it, and partaking largely of the attributes of common inflammation, has its remedial indications accordingly. The case just recounted, seems to me from the primary symptoms, and from the alteration of structure in the parts affected, to have been one of phlegmonoid inflammation, and would have been cured by the treatment, had not the diseased action extended itself through the parietes of the colon, and by that means, produced a constant evacuation of fæces into the cavity of the abscess, which eventuated in incurable fistula and artificial anus.

Art. X. *Case of Dysuria.* By the late Edward D. Smith, M. D. Professor of Chemistry, &c. in the South Carolina College.

In the year 1808, when residing in the district of Pendleton, South Carolina, I was called to visit a woman about twenty miles from my house. On inquiry, it appeared that for about two years preceding, she had experienced considerable difficulty in passing her urine, which commonly came away by drops, occasioning severe local pain and general distress. She had sought for medical assistance several times, but had derived little or no benefit from it, and from constant suffering had been reduced from a stout and healthy appearance to a feeble and declining state: she had borne several children, but none since the commencement of her present complaint, nor could she assign any particular circumstance as giving origin to her malady. At first I directed some cathartic medicines, to be succeeded by the common diuretics; but finding from information a few weeks after, that no permanent benefit was derived, I paid her a second visit. At this time I prevailed with her to permit me to attempt the introduction of a catheter, in which I could not succeed, nor was I more fortunate with a small bougie. It is observed that on this occasion, the woman laid on her side upon a bed, and that I was not allowed to make an ocular examination of the difficulties. Being thus foiled, I directed, as a palliative, the exhibition of large doses of laudanum, and spirits of nitri dulcis, which gave some relief for several weeks. The complaint, however, became so distressing finally, that I was again sent for, and informed that I might act in any way that I pleased. Apprehending that there might be some mechanical obstruction, such as a small calculus in the urethra, which might require removal, I directed the patient to be laid upon a table as in the operation for the stone, and passing the fore finger of the left hand up the vagina, I introduced at the same time the end of a small probe into the orifice of the urethra. The probe stopped about midway of the canal, but the resistance to its passage did not indicate the obstruction to be caused by a solid body. No moderate force, however, could advance the probe, until, with the finger in the vagina, I raised up the end of it, and then it immediately passed into the bladder. The woman declared that she felt more free from pain, than for many months previous; and the urine began to trickle along the sides of the instrument. I then withdrew the probe, and introduced a short piece of a thick bougie which was suffered to remain for about five minutes, and after was withdrawn, the urine flowed in a tolerable stream. Q dilating the urethra, as much as could be conveniently done, there appeared to be fleshy excrescences in it, to which I supposed the dysury to be owing, and for which I believed mechanical compression for a short time would be an effectual remedy. As it would be inconvenient to wear a catheter, and as I could not procure a shorter silver tube in that country, I directed a piece of a very small and smooth cane or reed to be used for that purpose, securing it from slipping into the bladder by proper ligatures at the external extremity. The event answered my expectations; without the use of any other means, the complaint was entirely removed in a fortnight, the patient recovered her spirits and health, and in the course of a year another child was added to the family.

Thinking that there is something unusual in this case, and that it might, on some occasion, furnish a useful hint to other practitioners, I transmit it for publication.

ART. XI. *Case of Calculus in the Urethra of a Child five years old.* By the late EDWARD DARRELL SMITH, M. D. Professor of Chemistry, &c. in the South Carolina College.

IN the month of November, 1811, when I was resident in the district of Pendleton, South Carolina, a man from Franklin county in Georgia, came to me for advice respecting his son. He stated that the child was about five years old, that soon after his birth he was observed at times to pass his urine with difficulty, and that this affection continued to increase until about three weeks ago, when a total stoppage took place. The child appeared to be in extreme agony, violent fever supervened, and on application to a physician in the neighbourhood, some internal medicine was administered, which produced no relief. After three days' severe suffering, a small hole was formed in the urinary canal, near to the anus, from which orifice the urine flowed out, and an abatement of the symptoms immediately took place. All the urine came by drops through this opening: the neighbouring parts were excoriated;—the child had a constant slow fever, and was much emaciated. From this description I concluded that a calculus had been expelled from the bladder into the urethra, in which it was so fast wedged, that not a drop of water could pass, and that the fistulous orifice had been formed behind it. As I could not conveniently visit the child, I desired that it might be brought into my neighbourhood, which was not done for two weeks after. On its arrival, upon examination, I found that there was a fistulous opening in the perinæum, within a few lines of the verge of the anus; and I remarked that air seemed to come occasionally through this opening, which caused me to apprehend that there might be a communication betwixt the rectum and the bladder. Introducing a probe into the fistula, and passing it upwards, I ascertained that there was a solid body about half an inch above the orifice, and afterwards examining through the orifice of the urethra, I was satisfied that the obstruction was caused by the presence of a calculus. The scrotum appeared somewhat distended, and felt as if it contained a fluid, which I supposed might be an extravasation of urine into it. The operation for extracting the calculus was appointed to be performed two days afterwards; but on visiting I found the whole of the scrotum much inflamed, and that a suppuration had taken place in a depending part of it. An opening with a common lancet was made at this point, and there was some discharge of pus and bloody water. Saturnine applications were directed to the scrotum, and after a few days, the inflammation having nearly subsided, it was resolved that the operation for extraction should then be performed. On passing a probe into the wound of the scrotum, it was found that there was a direct communication between it and the calculus; upon which the idea was suggested, that, by enlarging this wound, the calculus might be extracted without making any fresh incision. This was accordingly done; and while the finger of an assistant was applied to the perinæum behind the stone, to prevent its slipping back, I applied a long and slender pair of forceps through the wound of the scrotum, laid hold of the stone, and drew it out without much difficulty. The little sufferer bore this painful operation with much fortitude, and the calculus, which I have in my possession, weighed upwards of two drachms, being of the size of a large hazle-nut, and very rough and friable.

This operation was performed on December 4th. On visiting, December 6th, I found the patient without much fever, but was alarmed to find that a communication had been formed between the rectum and bladder,* and that the fæces had passed through the wound of the scrotum. A moderate compression upon the perinæum was made, and an abstemious diet was enjoined, and on visiting two days after, I found the child much better, his fever entirely removed, and his appetite and sleep good. No more fæces had passed through the bladder, nor did the urine flow through any other than the natural passage. From this time the child continued to mend; and in a fortnight after the operation his parents took him home. Six weeks afterwards his father brought him on horseback a distance of fifty miles to see me, when I found that he was become a stout and ruddy boy, able both to walk and run, which he never could do before. The wound in his scrotum was entirely healed, and the fistulous orifice of the perinæum was much diminished in size, and no water passed through it. About a year afterwards I heard that the boy continued to do well, since which time I have had no intelligence of him.

REVIEWS.

Art. XII. *A new Theory of Galvanism, supported by some Experiments and Observations made by means of the Calorimotor, a new Galvanic Instrument: also a new Mode of Decomposing Potash extemporaneously. Read before the Academy of Natural Sciences of Philadelphia.* By Robert Hare, M. D. Professor of Chemistry in the University of Pennsylvania, and Member of several Learned Societies. Philadelphia, published by M. Carey & Son.

THE experiments with the Voltaic apparatus generally present us with the associated phenomena of electric attraction and repulsion, light, heat, chemical decomposition, and the shock or action on the animal fibre. In the different modifications of the instrument, however, the relative intensity of these effects is by no means the same. Thus, it was early discovered, that a different law is followed in the increase of the power of igniting the metals, and in that of giving the shock to animals, or decomposing chemical compounds. Fourcroy, Vauquelin, and Thénard found that a few large plates, as twelve of twelve or twenty inches square, cause the metals to burn with the greatest brilliancy, while the same plates divided into forty-eight produce on them but an inconsiderable effect. On the other hand, the twelve plates gave a feeble sensation, and decomposed water slowly, while the forty-eight exhibited these effects greatly increased. From these experiments it was concluded, that the power of igniting the metals depends principally on surface, and that the power of giving the shock, and of producing chemical decompositions, is chiefly dependant on number. But this conclusion must be considered only as a first approximation. The chemical action and the shock are supposed, without sufficient proof, to follow the same law, and the other effects of the pile are not taken into consideration. The following experiments by Ritter, which do not seem to have attracted sufficient attention, give us a much more extensive and satisfactory view of the subject. They are extracted by Murray from Gilbert's "Annalen der Physik."

"*A.* Ritter constructed a pile with 1000 plates of copper, zinc, and pasteboard moistened with a solution of common salt in a decoction of litmus and galls, (as this conducts better than a simple solution of common salt.) He divided these into ten piles, each consisting of 100 plates, and connected them together in the common way, into a single large pile. He hung on the upper plate (which was zinc) of each of these piles a leaf of gold, and began first by including 100, then 200, and 300 plates in the conducting circle. The maximum of combustion he determined by the magnitude of the perforation which the blunt extremity of the iron wire, used in connecting the extremities of the pile, burns in gold leaf attached to the *plus* side; or if the action were strong, by the size of the perforation burnt in a leaf of brass foil. This maximum, with the above arrangement, he found to be between the 200 and 300 plates; beyond these the spark, to external appearance, for some time seemed greater, but it continued to lose its energy on the gold leaf, until at length all the 1000 plates taken together scarce yielded a trace of the true combustion, and the effect of the spark on the gold leaf appeared to be only mechanical.

"The *chemical effect*, which was determined by the quantity of gas evolved from the decomposition of water in a glass tube, increased from 100 plates, pile for pile, yet always slower, and usually at 600 plates it reached its maximum; 700, 800 plates, and so forth, operated more weakly, and all the 1000 plates considerably weaker than the 400.

"With respect to the *shock*, its energy increases 100 for 100 to 1000, and then it required a strong resolution to overcome the feeling, so that its maximum, with the above arrangement, seems to be beyond 1000. From piles of the same kind of 1500 plates, the shock was insupportable; but still no maximum could be observed.

"*B.* He erected a pile of the same magnitude, with a simple solution of sea-salt. He found the maximum of *energy of the spark* at the 200th plate; at 300 its action began to diminish, and at length the gold leaf was only mechanically affected, and more so than in case *A*. The maximum of the *chemical effect* was at 500: 1000 was not more powerful than 300. No maximum of the shock was observable.

" *C.* He constructed a pile of 2000 plates of copper, zinc, and cold concentrated solution of sal-ammoniac.

" Here the *spark* had its greatest energy or *maximum* between 600 and 800 plates, and then from this began again to diminish.

" The *maximum of chemical action* was not to be observed within 2000 plates. It was supposed to be at 4000 or 6000 plates.

" No *maximum of the shock* was observed. It is absolutely impossible, even when the hands are dry, to bear the shock of 2000 plates. From analogy, the maximum will be at 18,000 or 20,000.

" *D.* He constructed a pile of 1000 plates, and moistened the pasteboard only with well water.

" A maximum of the spark was not accurately discoverable. He places it at 100; the maximum of chemical effect was above 100 plates; the maximum of the shock between 600 and 700; at 900 and 1000 the sensation was weaker. If the hands are wetted with a solution of common salt, the maximum of sensation is between 500 and 600; if wet with solution of sal-ammoniac, between 300 and 400."

It is to be regretted, that, in these experiments, Ritter should have neglected the electric repulsion, which would have been so readily exhibited by an electroscope, and which is so important in a theoretical point of view. It is also to be regretted that he should not have tried the effect of enlarging the size of the plates, without altering the number. These desiderata have, however, been in a great measure supplied by the experiments of other philosophers. Mr. Children constructed a battery of twenty pairs of plates of zinc and copper, each plate being six feet high by two feet eight inches broad. The heat produced by this instrument, when charged with a mixture of nitrous and sulphuric acid diluted with water, was so intense as to ignite six feet of thick platina wire, and to melt platina with great facility; but it was feeble in producing chemical decomposition, gave no sensible shock, and did not affect the gold leaves of the electroscope.

Dr. Wollaston formed what he calls an *elementary galvanic battery* of a single pair of plates, made at first of a very small size, but afterwards considerably enlarged. In this apparatus the calorific effect was still more completely insulated than in that of Mr. Children. The ignition of platina wire was produced with brilliancy, while all the other effects of the galvanic action, reduced to their minimum, were imperceptible. On the other hand, in the dry piles of Deluc and Zomboni, in which the size of the discs is very small, and their number exceedingly great, almost the only effects exhibited are the electric attraction and repulsion, and these exist to a remarkable degree.

The conclusion to be drawn from the above experiments is, that the power of the voltaic battery does not depend merely upon the number or upon the surface of the plates, but upon a certain relation between these; and that this relation itself varies according to the effect to be produced. The power of giving the shock is most dependent on number, so that increasing the number of the series adds to this effect; but one of Ritter's experiments shows that even here there is a maximum. The power of causing combustion, on the contrary, seems to be almost independent of number, so that this effect is produced in the most sensible manner by twenty, and even by a single pair of plates. For the other effects of the voltaic battery, intermediate relations between surface and number must be observed, in order to obtain the maximum.

These curious facts naturally lead us to look for their explanation, although, while the general theory of galvanism is itself so imperfect, we can hardly expect the subordinate theories to give us satisfaction. The following view of the subject, which we extract from the classical work of the late Dr. Murray, is that which I believe to be most generally received by chemists.

“ Fluids are much less perfect conductors of galvanism than the metals; hence from the extent of the series of imperfect conducting matter which it has to pass through in an arrangement composed of a great number of plates, its velocity is retarded; it is, as it were, accumulated, becomes more concentrated, or has its *intensity* increased. And as the shock is dependent principally on this intensity, as is proved by the shock from the Leyden phial, a certain degree of it being requisite to overcome the resistance of the animal fibre, a violent one is given from a battery constructed with this repetition, in other words, with plates, the number of which is great, compared with the surface. For the same reason a certain intensity of the galvanic principle will be requisite to overcome the resistance of liquids subjected to its action, all of which are likewise comparatively imperfect conductors, and hence the accumulation of it, from resistance in the galvanic series, will favour its action on these liquids. But in the application of galvanism to metals, as these admit it with comparatively little resistance, it is not requisite that it should be at the same pitch of *intensity:* the effect will rather be dependent on the *quantity* applied; and hence the large plates, generating and affording it with more rapidity, its effects on the metals, igniting and causing them to burn, will be increased.”

This view is still further explained in the following passage.

“ A certain degree of intensity in the galvanic fluid is requisite to enable it to penetrate imperfect conductors. This intensity is produced by its accumulation in the galvanic pile or trough, and this accumulation is obtained by the repetition of imperfect, compared with that of perfect conducting matter in the galvanic arrangement: of course, by the repetition of the plates, between each of which the fluid or humid disc (the imperfect conducting matter) is interposed; and as from the relative conducting powers of the cuticle, of fluids, and metals, a greater degree of intensity will be requisite to enable it to penetrate the first than the second, and the second than the third; so the maximum of effect on animal matter, in other words, of the shock, must, from plates of a given surface, be at the greatest number; the maximum of decomposition of fluids at an intermediate number; and the maximum of effect on the metals at the smallest number, compared with extent of surface."

We have now presented a brief account of what is known to chemists on the subject of the relation which exists between the number and surface of the plates of a voltaic battery, and the intensity of the different effects produced by it, particularly those of heat, the shock, and chemical decomposition. We have thought it proper to exhibit this view to our readers, because it is in itself interesting and important, and because it seemed to us necessary in order to understand the precise nature and merit of the "new theory of galvanism," and "new galvanic instrument," described in the work which we have now to take under consideration.

Professor Hare's "New Theory of Galvanism," consists in the opinion that "the principle extricated by the voltaic pile is a *compound of caloric and electricity*, both being original and collateral products of galvanic action." In this theory, the received doctrine of the identity of the principles evolved by the electrical machine and the voltaic pile, is denied; the former being made to consist of pure electricity unaccompanied by caloric, the latter of the same electricity combined with caloric. In the pile of a small number of pairs, the "calorimotive" power is predominant, and heat is the principal product; in the pile of a great number of pairs, the electromotive power predominates, and electricity is the principal product.

It must be acknowledged that this theory presents itself with an air of great plausibility. When we see ignition, fusion, combustion, produced by the action of a subtle fluid, what can be more natural than to conclude that this fluid is caloric itself? Now, in the voltaic battery of a few pairs, or of a single pair of plates, scarcely any other effects,

little intensity, as to pass through bodies with a very sl
and obstructed motion.[*]

Of these two opinions, Dr. Hare's certainly appears to
the more obvious; yet we are induced to give a preferen
to the old doctrine, and that for the following reasons.

First, We know that common electricity, like galvanis
will, under proper circumstances, cause the ignition a
deflagration of metals. To explain this fact, Berthollet
adopted the old doctrine of a *cold fusion*, and Dr. H
thinks that "the electric fluid," which, according to hi
does not itself contain caloric, "combines with the lat
caloric previously existing in the metal, and, adding to
repulsive agency, causes it to overpower cohesion." B
whatever may be the explanation, we cannot see why
should not apply equally well to the heat excited by the el
tricity of the voltaic battery; and it seems to us not ve
consistent with sound logic, to adopt different theories, wh
the facts are so entirely analogous.

There is a circumstance connected with the fusion of m
tals by common electricity, which it is important to con
der in the present discussion. When the accumulation
electric fluid is very great, if it be made to pass through
wire so fine as not to give it a free passage, the wire w
be melted; but if a larger wire be substituted, it will
only escape fusion, but will scarcely have its temperat
raised. It appears, then, essential to the heating power

also increased by causing the electricity to pass through imperfect conductors. The following experiments on this subject are extremely curious.

"Van Marum placed the bulb of a very sensible thermometer in a cavity in a brass plate, five inches in diameter, and eleven in length, which was suspended by silk strings near the conductor of his powerful electrical machine. Neither from positive nor negative electricity was there any rise of temperature, nor was any perceived when the bulb was placed in a cavity in a piece of charcoal. But if the bulb of the thermometer were suspended loose between two conductors so as to have the stream of electric matter directed upon it, it rose with a powerful apparatus eighty or one hundred degrees of Fahrenheit's scale, and this whether the experiment were made *in vacuo*, in atmospheric air, or in any other gas.* When a thermometer is placed between imperfect conductors, its temperature is raised. Nairne observed, that when the bulb was placed in the luminous current between two balls of wood, it rises thirty-two degrees; and Van Marum found, that in placing it in a cavity in a rod of wood, placed between the ball of the conductor and the conducting wire of his apparatus, it rose in five minutes from 61° to 112°."

Secondly, Common electricity is also capable, like galvanism, of effecting chemical decomposition. To this fact too, Dr. Hare seems to have thought it necessary to give a new explanation; and he supposes that the decomposition is produced, in one case, by "divellent polar attractions exercised towards the atoms," and in the other, "by mechanical concussion, or that process by which the particles of matter are dispersed when a battery is discharged through them." The idea that the atoms which are combined to form the integrant molecules of a compound, can be shaken asunder by mechanical concussion, is so strange and so entirely at variance with all we know respecting the power of chemical affinity, that we were surprised to find it even casually suggested by Dr. Hare, and are convinced that it cannot continue to retain his sanction. It is true, that in the power o producing decomposition, as in that of exciting heat, a grea difference is observed between the actions of common electricity and of galvanism; but we think that for the explanation of this, as of the other case, it is sufficient to have recourse to the well known difference in the intensities of th two fluids, without denying their identity. Let us conside for example, the decomposition of water. The intensity o the galvanic fluid is so small, that water conducts it ver imperfectly. If, then, a wire be introduced into water fro the voltaic battery, the emission of fluid will be confined t its extremity, where, according to the theory of the poin the intensity is the greatest; the wire acting in water, wit the galvanic electricity, as it does in air, with the commo electricity. In consequence of this, the fluid is accumulated at one point, and there the decomposition is effected On the other hand, water is known to be an excellent conductor of mechanical electricity; so that if a wire connecte with the machine be introduced into water, the electricity will be rapidly dispersed from every part of it, and its influence will be no where sufficiently powerful to produce decomposition. If this difficulty, however, be obviated, and the stream of electricity be made to issue from the extremity only of the wire, effects very analogous to those of galvanism will be produced. Dr. Wollaston devised an ingenious arrangement for this purpose. A silver wire, of very small diameter, was coated with sealing-wax, and cut through in the middle. The extremities of these portions of wire, thus exposed, were introduced into water at the distance of one-tenth of an inch from each other, and placed in the circuit of a common electrical machine, in action. The water was decomposed, as by the voltaic pile. Introduced into a solution of sulphate of copper, the wire on the negative side had a precipitate of copper formed on its surface; and, on reversing the direction of the current, the copper was re-dissolved by the power of the wire, now rendered positive, and the precipitate appeared at the end of the other wire. All these effects are exactly similar to those exhibited by the voltaic battery; and other experiments still more decisive were afterwards repeated by Sir Humphry Davy, with the same apparatus. It appears, then, that in order to make the chemical agency of mechanical electricity similar to that of galvanism, it is only necessary to obviate the effects of its excessive intensity; and this might, perhaps, be more perfectly accomplished than by the apparatus of Dr. Wollaston, by using wood, or some other imperfect conductor, instead of wire. In some cases, indeed, the intensity of the electric fluid may offer no difficulty to its chemical action, as, for instance, when applied to dry ammoniacal gas, which is a non-conductor, and which presented the first example of electrical decomposition. I have no where seen, that, in this form, ammonia has been decomposed by the voltaic apparatus, and I even doubt whether this would be practicable.

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* Philosophical Magazine, vol. iii. p. 193.

Third, Even the peculiar shock given by the voltaic apparatus, may be imitated by mechanical electricity. To do this, it is only necessary to communicate a very small charge to a Leyden battery. It is known, that, in this case, the electric intensity is very low, so that the electroscope can scarcely be affected by it; yet the quantity of fluid may be considerable. We have procured then the very circumstances which we have supposed to give rise to the peculiarities of the galvanic electricity, and it is remarkable that the shock which is given, is scarcely to be distinguished from that of the pile. Could the discharge be, by any method, rendered continuous, it is probable that the analogy would be perfect in every respect. Another method of imitating the sensation given by the pile, and which also serves to confirm the common theory, is by receiving the shock from an electrical battery through an imperfect conductor, such as a piece of pointed dry wood.

In the foregoing paragraphs we have assumed galvanism as the object of comparison, and have shown that all its prominent features may, under proper circumstances, be exhibited by common electricity. We might now invert the proposition, and show that what have been deemed the peculiar properties of common electricity may all be displayed by galvanism; but we avoid this topic because it is amply treated in the works of chemistry. We shall conclude this subject, therefore, by considering two of Dr. Hare's arguments, one in favour of his own hypothesis, and the other directed against ours.

"The effect of the galvanic fluid on charcoal," says Dr. Hare, "is very consistent with my views, since, next to metals, it is one of the best conductors of electricity, and the worst of heat, and would therefore arrest the last and allow the other to pass on." Now that this is not the case, when the electric intensity is at all considerable, is proved by the well known fact, stated immediately afterwards by Dr. Hare himself, that charcoal is "peculiarly liable to intense ignition when exposed between the poles of the voltaic apparatus;" and we have no where met with any experiments to show, that "it does not display this characteristic with common electricity." Dr. Hare has indeed proved, and it is a new and interesting observation, that where the electric intensity is very low, as in his battery of two large galvanic pairs, "the thinnest piece of charcoal intercepts the calorific agent, whatever it may be;" but to make this of any importance, as an argument in favour of his theory, he ought to have shown, at the same time, that "the electricity was allowed to pass on." This he has not done, and in all probability, it was not the fact.

Dr. Hare's objection to the received theory is given in the following terms. "The opinion of Dr. Thomson, that the fluid of the pile is in quantity greater, in intensity less than that evolved by the machine, is very inconsistent with the experiments of Dr. Wollaston, who before he could effect the separation of the elements of water by mechanical electricity, was obliged to confine its emission to a point imperceptible to the naked eye. If already so intense, wherefore the necessity of a further concentration? Besides, were the distinction made by Dr. Thomson correct, the more concentrated fluid generated by a galvanic apparatus of a great many small pairs, ought most to resemble that of the ordinary electricity; but the opposite is the case."

The explanation which we have already given of the action of Dr. Wollaston's apparatus, contains, we think, the answer to the first part of Dr. Hare's objection. As to the assertion contained in the second, we conceive it to be entirely erroneous, and, to prove it so, we need not go further than the work itself, for, on turning over the leaf, we find the following sentence: "I had observed that as the number of pairs in Volta's pile had been extended, and their size and the energy of interposed agents lessened, the ratio of the electrical effects to those of heat had increased; till in De Luc's column they had become completely predominant." Dr. Hare indeed asserts, that "the ignition produced by a few large galvanic plates, where the intensity is of course low, is a result most analogous to the chemical effects of a common electrical battery." But it appears to us, on the contrary, that the fusion by the electrical battery is most analogous to that excited by a very extensive voltaic series, such as that used by Sir H. Davy, in as much as it is accompanied by the power of giving the shock, and of causing electrical attraction and repulsion.

Upon the whole, we think it clearly proved that mechanical and galvanic electricity are but modifications of the same influence, and consequently that the theory of Dr. Hare cannot be sustained.

But what is the nature of the electric fluid? may it not be itself a mere modification of heat, or of light? Is it a simple substance, or is it a compound? Such questions as these were formerly much agitated among philosophers, and their discussions may still be found in the works on electricity. For our own parts, we confess ourselves much inclined to the opinion, that electricity, as we commonly observe it, whether excited by the electrical machine or the voltaic pile, is a compound of a peculiar fluid, (electricity proper,) or perhaps of two peculiar fluids, (the resinous and vitreous electricities,) with caloric and light. The state i which these fluids exist in common electricity, is analogous to that in which light and heat are found in the solar rays; and if large and perfect conductors are not sensibly heated by the discharge from the Leyden battery, it is for the same reason that substances of perfect transparency are not heated by the passage of the sun's rays. Let the conducting power in the first case, and the transparency in the second, be diminished, and heat will be excited. On the other hand, we may suppose the state in which the constituent fluids exist in galvanic electricity, to be analogous to that presented by the union of light and heat from a culinary fire. In both cases the conductibility (if we may use the term) is diminished. The passage of the electricity is now obstructed even by the metals, and that of the fire-beams even by the clearest glass, and heat is produced by both. This view is certainly at least plausible; yet we do not mean at present formally to adopt it, and our principal motive for presenting it here, is the hope that it may prove a sort of neutral ground, on which the ingenious professor and we may be induced to meet.

Before we finally dismiss Dr. Hare's new theory, it is proper that we should make some remarks upon the subject of what he terms *calorimotion*. Volta supposed the electricity extricated by his apparatus to be the consequence of an *electromotive* power in the plates. Dr. Hare, believing caloric an "original and collateral product" of the pile, has had recourse to a *calorimotive* power to account for its production. In the theory of Volta, substitute *caloric* for *electricity*, and you have the implied theory of Dr. Hare. Let this be attempted; treat of the resinous and vitreous, or if you please, of the positive and negative calorics; consider the mode of action of the interposed humid bodies; pursue the analogy into its details; reconcile these views with the known properties of heat; and you will find that you have undertaken a difficult, perhaps an impossible task.

Still it is in our power to produce, by direct means, a true *calorimotion*, that is, a movement or circulation of caloric, from one part of a body, or system of bodies, to another;

and it is remarkable, that when we do so, there is, in many instances, a corresponding circulation of electricity. The following experiments seem to us to go directly to the proof of this point.

M. Schweigger* constructed an apparatus composed of fourteen small cups of copper, all filled with diluted sulphuric acid, and connected together alternately by copper wires and strips of paper moistened with salt water. Thus arranged, the apparatus produced no effect; but when every second cup was heated, and the intermediate ones left cold, the galvanic influence was plainly exhibited, water was decomposed, and the copper wires oxided. On the removal of the lamps, all action immediately ceased.

M. Dessaignes, also, has made a great number of experiments upon the same subject, and, in particular, has distinctly proved, that by heating or cooling unequally a homogeneous body, we develope in it the power of exciting muscular contractions in a prepared frog,—the most sensible of all electroscopes,—in the same manner as two heterogeneous bodies would do. Thus he has shown, that if a silver spoon be unequally cooled by placing one end of it in a mixture of salt and snow, we shall observe contractions in a prepared frog, whenever we touch the great nerves with the warmer end, and the feet with the colder. The same effect is also produced, in a very curious manner, by holding the frog by the feet in one hand, and touching the nerves by a finger of the other hand, previously cooled by an immersion of a few seconds in a freezing mixture. Again, if the nerves of the frog be plunged into a vessel of cold salt water, and the feet into another vessel of warm salt water, and we afterwards touch the water in the two vessels by a finger of each hand, we shall excite lively contractions at every successive contact†.

It will be observed, that, in all these experiments, we have, in the first place, a body unequally heated, and that it is while the caloric must be moving through the body, to restore its equilibrium, that the electrical effects are exhibited.

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* Journal de Physique, 1811. p. 105.

† Annals de Chimie, vol. iii. p. 418.

We have now, in the last place, to notice the "New Galvanic Instrument," announced in Dr. Hare's book, and to which he has given the name of "Calorimotor." His description of it is as follows:—

"Twenty copper and twenty zinc plates, about nineteen inches square, were supported vertically in a frame, the different metals alternating at one half inch distance from each other. All the plates of the same kind of metal were soldered to a common slip, so that each set of homogeneous plates formed one continuous metallic superficies. When the copper and zinc surfaces thus formed, are united by an intervening wire, and the whole immerged in an acid, or aceto-saline solution, in a vessel devoid of partitions, the wire becomes intensely ignited; and when hydrogen is liberated it usually takes fire, producing a very beautiful undulating or corruscating flame." "The experiments have since been repeated and found to succeed by Dr. Patterson and Mr. Lukens, by means of two continuous sheets, one of zinc, the other of copper, wound into two concentric coils, or spirals. This, though the circumstance was not known to them, was the form I had myself proposed to adopt, and had suggested as a convenient one for a galvanic apparatus to several friends at the beginning of the winter," &c.

It is impossible to read the above description, without being convinced that Dr. Hare's calorimotor is *essentially* the same instrument as the elementary battery of Dr. Wollaston. It is, like that, a voltaic apparatus of two plates, and it is employed for the same purpose of igniting the metals. It differs from it, however, under both its modifications, in the arrangement of the plates, its dimensions are incomparably greater, and its effects are much more striking and brilliant. Indeed, the idea of increasing the effects of the galvanic experiment, by enlarging the size of a single pair of plates, without increasing the number of the pairs, may, we believe, be claimed by Dr. Hare, and was certainly first executed by him.

In the experiments made with the calorimotor, it was soon perceived that the effects were not increased in proportion to the increase of surface; and, in one of the spiral kind, of an immense size, constructed by Mr. J. P. Wetherill of the Philadelphia Academy of Natural Sciences, the maximum was actually transcended, and the heat produced comparatively inconsiderable. The obvious remedy for this inconvenience was to divide the sheets of metal, and to make an apparatus of two or more pairs instead of one. It was not, however, so obvious, that these divided pairs could still be retained in the same vessel; yet this was successfully done, by Dr. Hare, with two pairs, the electric intensity being so low, that the fluid itself acted the part of a non-conductor. Still, as the intensity will increase with the number of pairs, it will soon become necessary to place them in separate vessels, that is to convert the instrument into the ordinary voltaic battery. Upon the whole then, we consider the calorimotor as limited, both in the variety and in the extent of its powers. Its only sensible effect is in the production of heat, and even here it is confined to the heating of the metals, and is, in its nature, incapable of exhibiting that intensity of ignition witnessed in the apparatus of Mr. Children.

Art. XIII. *History and Description of an Epidemic Fever, commonly called Spotted Fever, which prevailed at Gardiner, Maine, in the Spring of* 1814. By E. Hale, jr. M. D. M. M. S. S. Boston, printed by Wells & Lilly, 1818.

No portion of Medical Literature has probably been cultivated among us with greater success, than the history of the epidemics incident to our climate. The works of Rush, Miller, Hosack, Caldwell and Irvine on yellow fever, are deservedly celebrated for extent of research, minuteness of observation, and in some respects, for the original and truly philosophical views which they exhibit.

Nor, perhaps, has the subject of our winter epidemic, been treated with scarcely less ability. The writings of Strong, of Gallup, of North,—the Report of the Massachusetts Medical Society, in relation to it, afford us a very interesting and instructive account of this wide wasting disease, in the several modifications which it presented in different sections of New England.

Of these publications, our readers are doubtless sufficiently informed. But the work now before us, is of a later date, and we have reason to believe, has been less circulated in this part of the country. We shall, therefore, probably render a service by calling attention more generally to it, and in doing so, we cannot forbear to express the very high sense we entertain of its merits. As a history of the disease it is exceedingly satisfactory. There is no encumbrance of extraneous matter to obscure the subject, and it claims the rare praise of a pure, perspicuous and condensed style. The manner, indeed, of our author, is so compendious, as nearly to preclude an analysis or abridgment of his work, and we shall, therefore, be content with merely acquainting our readers, that the disease, as he describes it, seems to have had its ordinary character of typhoid malignity, and to have been most successfully managed by moderate evacuations, in the first instance, of the stomach, especially when oppressed—then by sweating long and steadily sustained, with the stimulating diaphoretic medicines aided by the vapour bath—and finally, where there was a tendency in the case to sink, by cordials and tonics.

It is somewhat remarkable, that so little has been written on this disease, as it appeared in the middle, the western and southern states. Excepting some very slight notices contained in the periodical journals, we have been unable to meet with any accounts of it. To supply, in part, this deficiency, we propose to appropriate a few of our pages to the subject, and shall be highly pleased to receive from some of our intelligent correspondents, further communications concerning it.

In tracing the history of this epidemic, we find that so early as the year eighteen hundred and six, the attention of several practitioners, resident in different parts of New Hampshire and Massachusetts, was arrested by some sporadic cases of a fever, of a new and extremely alarming complexion. After lingering a short time in that part of the country, it gradually spread itself, till it became diffused in two or three years, over the whole of the New England States, and subsequently entered into New York and the Canadian provinces. At these latter points, it seems to have again paused, and then pursuing a direct track through the interior of this State, invaded Ohio and Kentucky, next occurred in the north-western part of Virginia, and finally extended to the southern extremity of the United States.

Its first appearance in Philadelphia was towards the close of the winter of eighteen hundred and thirteen. Of its approach, we were not warned by any of those monitory signs, which are commonly the precursors of pestilence. There was nothing remarkable in the condition of the weather, or in the nature of the existing diseases. It is true, that on the shores opposite to the city, and in some parts of the circumjacent country, we heard for several preceding weeks, of the ravages of an unusually mortal fever. Nevertheless, we were still healthy: and without any of the portentous indications of the cloud, which at the moment was gathering over our heads, prepared to burst its contents upon us.

The season being far advanced when the disease reached our city, it did not continue long among us, and, however frightful were its attacks in many instances, the aggregate of its mortality was not very considerable. But on its return the succeeding winter, it assumed a more envenomed form, its prevalence was greater, and the victims of its ravages infinitely more numerous.

It is not to be expected, that, within the narrow limits to which we are unavoidably restricted, we should descend into details, so as to exhibit a complete view of an epidemic, which has been very essentially modified by the operation of all those causes that influence the nature of diseases. It is our intention, on the contrary, to present a mere summary, pretty much the result of our observations and reflections on the disease, as it appeared in our own practice, or as it has occurred elsewhere, from communications we have received, and on which we can implicitly rely.

All accounts agree in representing the disease as a Proteus, assuming every variety of shape, and requiring no little diversity of treatment. But in whatever form it attacks there ensues a great, and in many instances, a sudden and unexpected prostration of strength. Connected with this extreme debility, there are, for the most part, alternate chills and heats. The skin is hot or cold in rapid succession—soon, however, becoming dry and pale, or mottled. The face is sometimes livid, or more generally seems, where the attack is violent, as if it were bronzed over,—the alæ of the nose are contracted,—the forehead has a smooth and polished surface,—the eyes are wild and glassy, and the physiognomy altogether of the patient, is expressive of an inconceivable degree of anxiety and distress.

The pulse which, at first, is slow and apparently depressed, becomes, after a few hours, quick, feeble and tremulous, like the vibrations of a small cord, and ultimately sinks so entirely, as not to be perceptible. Now and then, from the commencement, there is more or less affection of the head, sometimes partial wanderings, or errors of mind, which speedily run into wild delirium, and at last terminate in stupor and lethargy. But the invasion of this disease is occasionally even more sudden and violent. It is stated on indisputable authority "that amidst their labours and occupations, persons have been seized, and cloven down as if by a stroke of lightning." When the attack is lighter, it begins with pains in the great or small joints, even in the fingers or toes, in the side, stomach, back, neck, or head, and are acute or obtuse, so as to be compared to the sting of a bee, or to the pounding of a hammer on the part. These pains, which are for a short period, fugitive and unsettled, become ultimately fixed in the head, affecting the powers of vision in various degrees, from dimness of sight to utter blindness, and soon afterwards, delirium and coma supervene.

In another form of the disease, there is seemingly no local determination. Commencing with the same prostration of muscular power, the person complains of listlessness, some anxiety about the præcordia, and a slight tension of the forehead. There is neither chill nor heats, though the pulse is feeble, irregular, and quick, amounting to from one hundred and forty to one hundred and fifty strokes in the minute. This is as insidious a guise as the disease assumes. Being so destitute of positive symptoms, it is apt to lull suspicion, till a condition of things is disclosed of the most desperate nature. After a while pain in the head occurs, to which other sensations are added, such as vertigo, throbbing of the temples, a severe stricture across the eyes, sometimes attended with a morbid vigilance which nothing can subdue, and on other occasions by the profoundest somnolency. Delirium generally exists in various gradations, from mere incoherence of ideas, to the constant utterance of violent rhapsody. If, however, the patient continues in possession of his senses, his spirits are exceedingly dejected, and he is harassed by the solicitudes and apprehensions of death, sighs and complains constantly of the uncontrollable wanderings of his distempered imagination, which conjures up and presents the most hideous group of phantasms and hallucinations. It is, indeed, difficult to conceive a situation, in which a patient is more restless, agitated, and distressed, or that is better calculated to excite our sympathies and exertions in his behalf.

Next we are to mention a form in which it is ushered in with all the circumstances of pneumonic congestion or inflammation. Cases of this description were very common. They commence with chill and fever, accompanied by considerable pain in the side or breast, and, as might be expected, with a difficult, panting, and laborious respiration. There is here an unusual determination to the lungs, as is evinced, not only by the symptoms already stated, but by the incessant cough and bloody expectoration, by the flushed and tumid countenance, and by the red, suffused, and wandering eye, which are found regularly to exist.

In these cases, and especially as the disease prevailed to the south, and among the members of the medical class from that quarter of the country, there was always some gastric distress, and occasionally violent and unremitting vomitings of bile. The pulse, so far as we have had an opportunity of observing, was invariably full, voluminous, and strong, though *soft* and readily *compressible.* It had none of the tension or capacity of resistance which indicates inflammation.

In every leading feature these cases resemble very closely the ordinary bilious pleurisy of our country, and were a practitioner called to one of them, who was not conversant with its peculiarity, he would, most probably, decide at once to manage it as an inflammatory affection. This counterfeit character, however, does not last long. In a day or two, and often in a few hours, the appearances of increased action wear away, and the typhoid condition becomes distinctly marked and fully established. Muscular power, which from the commencement is not a little reduced, now rapidly declines, while all the pneumonic affections are aggravated, and the mind, which hitherto had preserved its tranquillity, becomes distracted, or the patient sinks into heavy and stertorous slumbers.

Most generally, if the tongue and fauces be inspected, they will be seen covered by a thick encrustation of a dark brown colour, arid and hard to the touch, and besides which there are cold extremities, haggard countenance, a damp livid surface, speckled with petechiæ and vibices, which confer the popular name of spotted fever on the disease. These, however, were exceedingly rare, occurring, perhaps, about once in a hundred times.

In this epidemic we occasionally met with cases in which the throat was apparently the chief seat of attack. They, however, were few in this city—though it appears that this was the most common shape which the disease put on to the south. Nearly all the cases which we had an opportunity of seeing in Virginia were of this kind.

There is nothing very peculiar in the onset. The early symptoms are those of an ordinary cold attended with debility. The throat, at first, is slightly affected, but little time elapses before we have cause for alarm. At a moment when not at all expected, we shall be surprised by a total sinking of strength, and by the equally sudden appearance of the most impeded respiration. This differs from all the anginose affections. There is no enlargement of the tonsils. But we find a considerable swelling of the membranous lining of the fauces, of a dark mahogany colour, and not florid, as in active inflammation. It is our impression that it is decidedly the most malignant form of the disease. It terminates life more rapidly, and under circumstances less to be anticipated.

What is this most extraordinary disease? To this question, so interesting in itself, it is not easy to convey a satisfactory answer. That it is not fever, according to the definition of the nosologists, in all cases, is most manifest. The shape which it sometimes assumes does not, indeed, present one single feature of febrile affection. It is characterized by no preceding chill, no acceleration of pulse, no augmentation of temperature, no apparent disturbance of any one function, and has no definite crisis, or stated period of solution. Cases of this kind, however, were not very frequent.

Dissection shows that there is much congestion in some instances, as well as extensive inflammation throughout the three great cavities of the body, of a weak erysipelatous species. The inflamed surfaces are livid, with partial erosions, as in the incipient stages of gangrene. In the head, as well as in the thorax and abdomen, the contents are loaded, and we almost invariably meet with extravasations of dark, thin, or grumous blood, exudations of imperfect lymph, and effusions of serous fluids.

Taking into view the whole of the phenomena of this disease, as exhibited as well in its exterior physiognomy as by inspection after death, there can be, we think, little hesitation in considering it as a variety of typhous action, differing, however, very materially from fever of this character as commonly described.

The leading and most conspicuous peculiarity of the disease seems to us to depend on its assuming, for the most part, the *congestive* as contradistinguished from the *inflammatory* condition.

Little has hitherto been written or understood relative to the former shape of morbid action. The French pathologists seem to have entertained some indistinct notions on the subject, which they vaguely expressed by the term *engorgement*, afterwards adopted by the late Dr Rush, from Clarke on fevers, where it first occurs in an English writer, without, however, much elucidation, or strenuously insisting on its importance.

It would appear, from a variety of considerations, that the *congestive* differs from the *inflammatory* condition in this, among other respects, that the veins are chiefly concerned in the *first*, and the arteries in the *second*. In all such cases, the balance of the circulation is destroyed, by undue accumulations in the venous system, and especially of the large viscera, as the lungs, the liver, the spleen, and of the brain. As a consequence of this, we have generally, from the recession of the blood from the superficies, a pallid skin, more or less cold, a weak and emptied pulse, much oppression, attended by great debility. But when the arteries are affected, the usual indications of inflammatory action exist, such as a vigorous pulse, great heat and excitement, and uniformly perhaps some local acute pain or uneasiness.

The blood also affords a criterion. In the inflammatory state it is sizy, or cupped, or florid, while in the congestive it is dark, sometimes grumous, and with little or no tendency to coagulate firmly.

These are the principal or leading *external* signs or symptoms by which we may distinguish the two opposite conditions of the vascular system, and which are abundantly confirmed by the phenomena disclosed on dissection. Thus, in the *congestive* cases, the large deep-seated veins, especially of the viscera, are invariably found loaded and distended with dark grumous blood, whereas in the *inflammatory*, the chief evidence of a disturbed or irregular circulation, is a preternatural fulness of the capillary vessels on the surface of the viscera, with a florid appearance greatly diffused, denominated the blush of inflammation.

It follows from the preceding view, that in the *congestive* cases, neither the pulse nor the state of excitement will serve as guides in practice. Consulting these alone, we should be discouraged from venesection, at the very moment when it is urgently demanded by the loaded state of the veins. But though thus required, blood, under such circumstances, must be, in some cases, very cautiously detracted.

In concluding these remarks, we shall only further mention, that in all our speculations relative to the diseases of the circulation, we have confined our attention too exclusively to the arteries, even to the total neglect of the veins. But independently of the facts which we have detailed when it is recollected, that, according to the computation of the best physiologists, the veins contain nearly three-fourths of the whole mass of blood, they have the strongest claims to notice in all our reasonings on the subject.*

Diseases not wholly dissimilar to the one under review have been noticed. Consulting the European writers, we shall find records of epidemics approaching the same nature, and particularly in the shape of spotted fever. The works of Sydenham, and more expecially those of Huxam and Sauvages, contain some of the histories of the diseases to which we allude. The parallel is certainly not complete, though perhaps as much so as between any other two diseases of the same sort, prevailing in different climates, and among a different people. That form of it in which the pulmonary organs are chiefly concerned, is more accurately delineated by Sauvages, and on which he has conferred the title, since adopted very much among us, of *peripneumonia typhodes.* Nor has it been wholly overlooked in this country. By Dr. Bard of New-York, a malignant pleurisy is described closely resembling it, which prevailed in Long Island in the year 1749, and we are not without traditional evidence, at

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* Armstrong on Typhous Fever.

least, of the same disease occurring occasionally in ot
sections of the United States.

Of the causes of the disease little is ascertained. In co
mon with all other epidemics its origin is involved in do
and obscurity. As yet we know only that it commences
cold weather, and is generally dissipated by the warmth
Spring. It would seem, therefore, to be somehow depe
ant on a low degree of temperature, and this hypothe
receives no slender support from the fact of a fever nea
allied to it being actually produced in this way. The c
which we mean is very commonly to be seen among
paupers in the Alms House of this city, who are not unf
quently brought into that establishment in a state of tor
dity, by exposure to cold. From the extinction of sensib
ty and excitability, it is exceedingly difficult to arouse
system by any description of remedies. But where reacti
does take place, the result is a slow and feeble state
fever, very analogous, as we have stated, in all its essent
attributes and prominent features, to some of the forms
our epidemic.

Like that disease, in its more advanced stages, there a
low delirium, the dark and encrusted tongue, the skin is h
and parched, the pulse quick and small, the eyes glass
with dilated or contracted pupils, and the same lank, ha
gard, and distressed expression of countenance. But, on t
other hand, it may be alleged, that cold alone cannot be t
cause, since it does not invariably produce this effect, an
that the disease occasionally prevails during mild, and eve
warm weather.

The epidemic which lately prevailed so extensively i
the British Isles is ascribed by some of their best writers t
the low and penurious diet, and the squalidness and filt
resulting from the singularly distressed condition of th
country, since the general pacification of Europe. That, i
some respects, a very considerable similitude exists in th
two diseases is sufficiently apparent, and they are not with
out evidence abroad in support of the hypothesis, though
the same time we confess, it seems to us still to want confi

mation. Could such a state of things alone be the immediate efficient cause of the disease, or, in other words, were poverty and wretchedness adequate to the production of it, there are some of the sections of Europe which would be perpetually exposed to its ravages. As relates to our happy and abundant country at least, there can be no foundation whatever for any supposition which would associate disease with starvation and distress.

In this dilemma we are afraid we shall be constrained to resort once more to the gratuitous notion of a depraved or morbid constitution of the atmosphere, to help us to an explanation. That the disease neither originates in contagion, nor is spread by it, though there is some reason to suspect it to be occasionally the case, seems clearly to be proved by the universality of its prevalence, as well as by its observing the general laws of epidemics. This is more especially manifested by its compelling all other diseases to acknowledge its supremacy, and to put on its attire. During its existence in this city, and we have understood it was equally so as regards other places, every variety of morbid affection, whatever might be its original nature, received a complexional hue from its dominant influence, and exacted pretty nearly the same sort of treatment. The lancet, and other depleting remedies, were generally abandoned by our practitioners, and stimulants and cordials substituted. The appearance indeed of this epidemic wrought a complete revolution in the practice of medicine throughout the United States, from which, so far as we have been able to ascertain, we have scarcely recovered. Even at the present moment, in this city, the usual cases of winter disease are approached by some of our practitioners with the lancet, not wholly without trepidation and anxiety.

We have said, that in some instances we had reason to suspect contagion. They, however, were few, and by no means conclusive. With some of our medical friends, whose opportunities of seeing the disease were ample, no doubts whatever exist on this subject. By those who most strenuously support the hypothesis of contagion, some very strong adduced, derived chiefly from the disease as it peared among our troops on the Canadian lines and elsewhere. Thus it is stated, that the disbanded militia, on returning home, might be traced by the propagation of the fever along the road. Even admitting this, which we are not at all disposed to deny, we have reason to believe the disease was a distinct one, the typhous fever of camps, which is confessedly contagious. To imagine, indeed, that n epidemic which in a few years swept entirely over this immense continent, and thence, as we have some grounds to suspect, extended to Europe, producing every where great mortality, should be propagated by contagion, is to us altogether improbable and absurd.

The effect is utterly disproportioned to the cause. Diseases originating in contagion are always slow in diffusion. To this general rule we know of no exception. The small-pox has this property in the most eminent degree, and it would require a century to spread itself over an extent of surface which was infected in a few years by our winter epidemic.

As regards the exciting causes of the disease, there is no uncertainty, as they are the same which operate in all other epidemics not contagious, and consist of errors in diet, exposure to sudden changes of temperature, fatigue, watching, anxiety of mind, and whatever else has a tendency to produce a slight derangement of the system.

Two modes of treating the disease have been proposed. By one set of practitioners, the most profuse use of direct stimulants, as wine, brandy, carbonate of ammonia, camphor, has been recommended, while by another, the sweating plan was decidedly preferred. We have had opportunities sufficient of comparing these two modes of managing the disease, and we have not the slightest doubt of the superiority of the latter. The mind of the physicians of this city particularly, is pretty well made up on this point, and they all seem to acknowledge the infinitely greater success which atended the early and steady exhibition of the diaphoretic remedies.

Of the best means of exciting perspiration practitioners a so well informed, that we shall merely remark, that in our own practice nothing answered so well in this disease as the Dover's powder, repeated about once in three hours, strong wine whey freely given, and hot fomentations applied to the lower extremities, to the trunk, and to the arm-pits.* It may be affirmed, that where sweating is early induced, and perseveringly continued, recovery is highly probable. Death seldom indeed took place under such circumstances.

As the disease advances, or if we are called in the latter stages of its career, we are to combine with diaphoretics the intrepid employment of that class of remedies properly denominated cordial stimulants or incitants. The carbonate of ammonia is here "ipse agmen," we had almost said, the "unicum remedium." It is to be given with a fearless hand. Not less than five, and sometimes ten grains are demanded every hour. As co-operating means, strong wine whey, hot toddy, or Madeira wine, should be copiously used. It is at this conjuncture, too, that the spirit of turpentine may be exhibited with the greatest probability of advantage.

But should these measures fail, and there be a rapid sinking of the pulse, and other indications of extreme prostration of power, we must apply, as a dernier resource, to the surface in various parts, a decoction of cantharides and spirit of turpentine, so as to excite vesication, or the more irritant rubefacients, as a mixture of Cayenne pepper with brandy.

This is the practice in the simple forms of the disease. Distinguished, however, by great local determinations, as in the bilious, pneumonic and anginose cases, it seems to be universally conceded, that some difference of treatment is required. Emetics here have been found eminently useful. They are prescribed, as well to evacuate the stomach of its contents, as that salutary impressions may be made through the system generally. To be serviceable, they require be repeated several times in some instances, and the most ctive articles, as tartar emetic, should be selected. These are to be followed by mercurial purgatives. The James's powder answered well. It was originally employed at our suggestion, and displayed, when largely given, the combined operation of an emetic, purgative, and diaphoretic.

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* It was universally admitted, that the diaphoretics into which opium enters largely, were incomparably the best suited to this case. Even from opium by itself the best effects were often obtained, and particularly where much pain of the chest existed.

After the end is attained for which evacuants are administered, the case is then to be subjected to the same course of stimulating diaphoretics as in the other states of the disease, and if there be any topical congestions or pains, blisters become the appropriate remedies. They are especially indicated in the anginose and pneumonic affections, and in delirium or coma, and ought to be large enough to embrace the neck, or cover the chest or head, as the case may demand.

As relates to the use of the lancet, much controversy has prevailed. Conformably to our own experience, we should say that it was rarely required, or could be safely employed—and such was the prevalent sentiment among our best practitioners. By the immense force, or the peculiar nature of the remote cause of the disease, the vital energies were so crippled as not to allow of the detraction of blood. Even when the system was manifestly labouring under immense oppression, from congestion in the viscera, and particularly in the pulmonary organs, venesection afforded no great relief, and was often followed by an alarming depression of strength. The local powers of the circulation had become so enfeebled as not to move on the blood, so that it accumulated, and the part was firmly impacted. Bleeding here only served to reduce the quantity in the circulating vessels, without at all unloading the engorged organs. By cups, however, immense good was done in many instances, and especially when followed by the vesicating applications. As in the peripneumonies of very aged people, the stimulating emetics also proved serviceable, by emulging the vessels, and re-establishing a just equilibrium in the circulation.

Notwithstanding what we have said against venesectio let it not be understood that we extend our objections to the practice in the disease as it appeared in other places. We have, on the contrary, the most satisfactory evidence in our possession, that in several parts of the United States, the remedy, urged to a considerable extent, was imperatively demanded. The disease in the interior of this State, put on very generally an inflammatory character, and submitted only to depletion in its various shapes—and the same, I have understood, happened in portions of Vermont, of Virginia and Tennessee. In this, we have a fresh illustration of the truth of the influence of climate or position or other localities, in the modification of disease, and from which a lesson of importance may be deduced, to watch these fluctuations, with a view to the proper adaptation of the treatment, under the varied circumstances.

Distinct in almost every thing which relates to it, the ordinary prognostics are not applicable to this singular disease. Numerous are the cases of persons suddenly perishing with every sign of convalescence. The pulse here, will often totally fail as a guide. There are, however, certain symptoms which indicate great danger, among which, some of the least equivocal may be derived, by an experienced practitioner, from the contemplation of the countenance and habitudes of the patient. The countenance in this state, is marked by a very peculiar expression of wretchedness, or is exceedingly placid and inanimate, with the skin polished and of a leaden or bronzed complexion. This latter is a most alarming appearance, which will sometimes occur in a very early stage of the case, and we do not recollect a single instance of recovery where it existed.

As presented, such is the brief history of a disease, which originating sixteen years ago in the eastern extremity of our country, moved on progressively to the south, producing every where in its way, terror and devastation. In its fell career, it twice, as we have seen, invaded this city, and on each occasion, swept away some of the most valuable of our citizens. Long will it be remembered by us, as having caused the death of three of the most illustrious of our medical ers, Rush, Wistar and Dorsey—and every time of its ccurrence we were called to mourn over the loss of some of e members of the medical class, with whom it prevailed xtensively.

The disease cannot be considered as necessarily a very fatal one. By pursuing the practice we have inculcated, it roved by no means unmanageable among us. Compared with the yellow fever, this was certainly true. The number of deaths was proportionally much less. To attain success, however, in such a disease, the most steady and persevering use of the remedies is required, aided by every possible convenience and comfort. Death, in a majority of the cases which we witnessed, could be distinctly traced to the want of these advantages.

As an epidemic, most fortunately this terrible disease no longer prevails. No cases, except now and then a sporadic one, are to be met with in this city, and we have reason to believe it has become equally extinct in every section of the United States. To it has succeeded, in a great measure, the old inflammatory constitution of diseases, which are of a nature infinitely more intelligible, and managed, comparatively, with ease and certainty.

Art. XIV. *Observations on the Nature and Treatment of Marasmus, and of those diseases which may be strictly denominated bilious.* By Joseph Ayre, M. D. London, 1818.

To investigate the nature of those agents which derange the economy of the human system, and discover the connexion with the diseases and distress of which they are productive, is undoubtedly one of the most exalted duties that man is called upon to perform. There are so many disorders to which the human system is liable, under such varied forms, and apparently attributable to so many causes, that notwithstanding the exertions of the large number of ingen ous individuals who have laboured in the field of medic philosophy, we have, frequently, to attack a disease without knowing its vulnerable point.

No physician of the present day, we presume, would be disposed to question the propriety of the maxim which teaches us to remove the cause, as the first step in the cure of a disease—and he who succeeds in enlarging or correcting our pathological knowledge, is entitled to no small praise.

The disease in question, is well calculated to attract our attention, from its great prevalence among infants, and the insidious manner in which it generally undermines the constitution of its helpless victim. The opinions concerning its nature, and consequently the proper method of treatment have been various.

Notwithstanding the large collections of offensive matter in the intestines, and its unnatural appearance when evacuated, the morbid contents of the stomach in the early stages of the disease, and the relief afforded by emesis, our author does not suppose that the alimentary canal is the original seat of it. "But," says he, "upon whatever hypothesis we may found our explanation of the origin of marasmus, or the nature of those morbid states of the stomach and bowels which lead to it, or of the precise modes of action of those remote causes, which are found to act successively in increasing or extinguishing it, it appears to me indubitable, that its seat is always in the liver, and that the means directed for the relief of the deranged actions of this organ, are adequate to restore the other disordered functions to a healthy state. It has of late, I believe, been generally supposed that the marasmus of children, consists in a merely loaded and constipated state of the bowels. This notion, however, of the nature of the disorder, appears to me irreconcileable with the phenomena exhibited by the complaint, and with the observed effects of the medicines found useful in removing it."

That this *notion* has generally prevailed is well known, and Hamilton of Edinburgh, in whose essay on the use of purgatives, we have, perhaps, the most recent European repon this disease, does not appear to entertain any er. He has, indeed, given the preference to calomel er all other cathartics in marasmus, but it is *only* as a cathartic, without any comment upon its alterative effects.

Among us, however, this antiquated *notion* has long since ceased to exist. The agency of the liver in the production of the various symptoms of marasmus was not unknown to the American faculty, many years ago, and the plan of treatment which our author recommends, had become common long before the appearance of his "Practical Observations," &c.

Calomel in small doses, such as the fourth, sixth, or eighth part of a grain, frequently repeated, with the occasional exhibition of a cathartic, is the main part of our author's practice. We presume that nothing more will be necessary to establish the priority of our claim to this improvement than the short extract annexed, from an essay published by Professor Miller of New York, in the Medical Repository, twenty-two years anterior to the date of Dr. Ayre's work.*

The whole object of the work before us, is to prove that marasmus is a bilious disease—that it originates from hepatic obstructions, and that mercury is to be considered as its natural and legitimate remedy.

The author arrives at these conclusions after a train of reasonings marked by very unnecessary prolixity. We shall, however, endeavour to give the outline. The food, he supposes, when taken into the mouth, from its peculiar irritation, excites the secretion of the saliva, with which it forms a compound calculated to stimulate the stomach into its proper action—the chyme, in like manner, is the proper stimulant to the liver, and the bile performs its part in rendering the nutritive mass equal to the production of this succession of sympathetic actions. On this principle it is evident, that when one part of the digestive process is not performed, or partially executed, all the succeeding operations will be confused. But our author admits of an inversion of this order, between the stomach and liver, the former sympathising with the derangement of the latter, though in the "*recapitulation*" of his arguments, it appears that the stomach is referred to as the *primus ægrotus:* for, after supposing that marasmus consisted in a deranged and imperfect secretory action of the liver, and that the black stools and hematemesis were occasioned by discharges of blood from the extremities of the vena portarum, regurgitating into the stomach in one case, and passing *per anum*, in another—he adds, "this derangement in the function of the liver, commonly arises from a disorder commencing in the stomach," by which "an imperfect stimulus is given to the liver."

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* "Note.—A child of two years old may take a pill composed of one sixth part of a grain of opium and one third part of a grain of calomel, every second, fourth, or sixth hour, or sometimes oftener, according to the urgency of the symptoms," &c. Again, "It results from all this, that in calomel alone we possess an excellent evacuant in *the diseases denominated bilious;* and that in calomel joined to opium we have a medicine of still higher value."

In the same page also he further mentions the external application of mercury, as promising advantage in certain forms of the disease.

Whatever credit may be due to Dr. Ayre for his industry in observing the phenomena of this disease, of which he has given some proof, it is evident that neither his theory nor his practice is new. The single paper of Professor Miller to which we have already alluded, contains the outline and all the important particulars of the doctrine—and every one who is in possession of the lectures of the present Professor of the Practice, in the University of Pennsylvania, can satisfy himself of the fact; that it has been publicly taught in this country.

Professor Miller, indeed, has not diffused his ideas throughout so voluminous an essay, as that of our author, nor has he used such an unnecessary and reiterated train of argument to prove the agency of the liver in marasmus. But the identity of the opinions of these two authors may be seen in the single expression which we have quoted above, in which, in speaking of calomel as a remedy for marasmus, Professor Miller classes the disease with those "*denominated bilious*,"—the very expression that our author has copied in the title of his book. It may be seen also by the plan o treatment which Professor Miller recommended in the yea 1797, and which is virtually the same as that which is attempted to be introduced at this time, that the present author is merely entitled to the credit of having made a judicious selection of a method of practice, the best that ha hitherto been offered, and of compiling a work which ma be serviceable to many of its readers, and, perhaps, generall so in the country where it originated.

Marasmus is a disease of great prevalence, and particularly interesting as an object of investigation, inasmuch a it generally attacks those who are too young to assist th physician in discovering the cause of the complaint. N symptom, therefore, that can be made evident to the practitioner, should be disregarded, and it is superlatively a desideratum, to have a leading and peculiar indication of thi sort, to guide us in all our prescriptions.

That this is attained with respect to marasmus, ou author appears to be satisfied. The leading symptoms are, enlargement of the abdomen, unnatural alvine discharges, and an appetite not commensurate with the requisite quantity of nutriment. These, he thinks, point to derangements in the healthy economy, which designate the cause of the disease.

In addition to the part which the bile performs in the process of digestion, it gives a decided and peculiar colour to the fæces, by the absence of which we suppose that a diseased state of the liver is indicated. There are several ways of accounting for the fæcal discoloration. The first is, by supposing a superabundance of bile; secondly, too small a quantity; thirdly, a bilious fluid not properly composed; and lastly, a total absence of the hepatic secretion. It is upon these data that our author adduces the appearances of the *ejectæ*, in support of the doctrine of the hepatic origin of marasmus.

It is a fact which we believe cannot be denied, that the colour of the fæces depends, in a great measure, upon the causes above enumerated: we may, therefore, give our assent to the doctrine of hepatic derangement, when the alvine discharges are destitute of their natural colour, unless, indeed, some other cause is known to exist, which would undoubtedly effect the change. If we add to this, that mercury is found, by long experience, to be pre-eminently useful in the removal of the disease, and that during its exhibition in marasmus depending upon a paucity or vitiation of the bile, a bilious diarrhœa is a common occurrence, it would appear that the disease should be attributed to a derangement in the functions of the liver.

But however plausible this doctrine may seem, it is not established by any undeniable fact, nor can other hypotheses be superseded, by any train of argument deduced from the known phenomena of the disease.

The observation of Dr. Ayre respecting the successive operations of the various digestive functions depending upon each other, is probably well founded, and very applicable to marasmus, though not as he would explain them. He supposes a reversion of the natural order, and thus renders his fundamental principle useless; adopting in place of it nothing more than the indefinite principle of sympathy.—We advocate the gastric pathology of marasmus.

Any one who reflects on the vast changes effected in chemical compounds by an apparently insignificant alteration in the proportion of their constituents, will admit, without hesitation, that if the first process of digestion is improperly performed, the same changes cannot take place by the subsequent operations, because the same principles do not meet under similar circumstances. We are aware of the repugnance that many feel towards the idea of chemical actions taking place in the living body. Though numerous operations that would occur in the dead body are restrained by the influence of vitality, especially such as tend to the disorganization of the fabric, yet we should not too hastily deny the possibility of changes in the living state from the sole agency of chemical principles.

Do we not prescribe carbonate of potash followed by an acid? and is not the carbonate decomposed in the stomach the strictest principles of chemistry and physiolo acid is one of the results of those changes which tak in vegetable matter exposed to heat and moisture. such matter is introduced into the stomach it is expo both these agents, and very nearly to that extent whic most favourable to its decomposition. This tendency, h ever, is overcome by the stomach, when that organ is in full possession of its natural powers. Whether this is o sioned by the quantity or proper constitution of its s tions, or by some occult property with which we are u quainted, it is unnecessary to determine.

But suppose the stomach to be debilitated, as it frequ ly is—then it is unequal to the task of counteracting effects of chemical affinity. Chemistry, therefore, ass her empire, and the matter introduced into the stom instead of becoming perfect chyme, is a heterogen mixture of which an acid is likely to form a large pro tion.

These arguments, it may be said, favour the obsolete i of fermentation in the stomach.—It is true; though they not intended to defend that absurd hypothesis, by which mysteries of healthy digestion were explained.

Chemistry teaches us that common fermentation can ver produce a substance analogous to either chyme or ch But we do advocate the possibility of the acescent ferm tation in the stomach, when that viscus has lost its ous eructations accompanying the use of vegetable diet, which seem to indicate some process in the stomach not far remote from a natural decomposition of vegetable matter, as a large quantity of such gaseous matter is always a product of fermentation.

But the particular process by which an acid may be produced in the stomach, is not immediately necessary to our purpose. It is sufficient for the main point in our theory, that such substances are sometimes present in that viscus, and that their presence is attributable only to some process inconsistent with the natural one of healthy digestion.

In tracing the progress of digestion, we will suppose that instead of healthy chyme, a mass containing uncombined acid passes from the stomach into the duodenum. It there meets with the bile, which is a fluid decomposable by the acids. Though there is little difference in the constitution of this fluid secreted by the various warm-blooded animals, yet, from the great difference that may be effected in compounds, by a slight difference in the proportions of their constituents, the human bile is much more easily decomposed by acids than that of any other animal. It is remarked by Professor Thomson, who appears to have devoted a great deal of attention to animal chemistry, that all acids decompose the human bile, and, indeed, makes this the distinguishing mark between it and the corresponding secretions of other animals. What then is more natural than to conclude that the bile may be destroyed by the contact of the gastric acid?* And here let us observe, that if any objections may be offered to the idea of the formation of an acid in the stomach, from a mere chemical process, they will not apply with equal force against the probability of such a process taking place in the duodenum between the acid and the bile.

Upon the supposition of the partial or total destruction of the bile by the gastric acid, we have a ready solution of all the phenomena of a paucity or absence of that fluid, as indicated by the appearance of the fæces or otherwise.

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* We do not mean to coin a new chemical term, but for the sake of convenience merely to denote the acid formed in the *stomach* by the term *gastric*, whatever its constituents may be.

The alterative effects of calomel in removing the symptoms of marasmus, may be objected to our theory, upon the supposition that that medicine acts upon the liver, and in case of a derangement of the functions of this gland, producing unhealthy bile or suspending the secretion, the calomel restores it to a healthy action. But the alterative action of calomel is by no means confined to the liver. Its beneficial effects upon the stomach, when given in minute doses, has been as satisfactorily demonstrated, as its action upon the liver. We are authorized to say that it has been a favourite prescription with Dr. Physick and the late Dr. Kuhn of this city, for many years, in cases of gastric derangement.*

The occurrence of a bilious diarrhœa, with which the symptoms of marasmus usually disappear, although adduced by Dr. Ayre in evidence of the hepatic origin of that disease, is just as easily explained by the gastric theory, since, if the production of an acid in the stomach is prevented, the bile will no longer meet with a destructive agent: and instead of a bilious diarrhœa being a proof of a renewed secretion from the liver, it may with equal plausibility be ascribed to the absence of that cause which hitherto had destroyed it as fast as it was produced.

It is a happy circumstance, however, that whichsoever theory we may adopt, with respect to the primary location of marasmus, our plan of treatment will remain the same; and it is a matter of congratulation, that we now possess a remedy for that destructive enemy of our species, sanctioned by theory and tested by the infallible ordeal of experience.†

H.

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* The practice alluded to consists in the exhibition of the fourth, sixth, or eighth of a grain of calomel every half hour or hour, to quiet the irritability of the stomach, more particularly in cases of bilious vomitings.—*Editor.*

† The above review has been contributed by an ingenious correspondent. We give it an insertion without assuming responsibility for the views it embraces, several of which differ essentially from our own.—*Editor.*

s, throws about
Art. XV. *An Inaugural Essay on Mania a* by even leaping
Snowden, of Philadelphia—18 ing the utmost
ms and terrible
THE theses issued from the medical sc n create, to ha-
vania are characterized, for the most part,
ability, and we sincerely regret the suppre us precisely such
lation which made the publication of the nticipated, from
would not be difficult to point out, and i
of this sort has been actually made,* a the stomach was
number of these essays that might be read diseased, some-
the most enlightened member of the profe d, sometimes by
are eminently deserving of a place in ever tory marks were
To these inquiries we are indebted in pa
ly all the exact knowledge we possess of o ery conspicuous
dicinal vegetables, as well as for some ver isease, as indeed
coveries in physiology, and occasionally e ion with the sto-
nature strictly practical have been success
Among the dissertations of the latter de sted, and in other
not help considering the one of which w nd extravasations
offer some short account to our readers. It egards either of
first place, in order to give due authority
mention, that for several years he was at of the immedi-
dent student, to the most extensive of ou n all drunkards,
and that he had ample opportunities of o rtions, and even
ease, and determining the best mode of m al occurrence.
Drunkenness is the vice of our country , some difference
and with, perhaps, sufficient accuracy, that superficially, we
sume forty millions of gallons of ardent and other indica-
might be imagined, we have every variety. But experience
in a correspondent degree of which such eceptive, and, ex-
these pernicious liquors can be productive are to adopt the
Connected with the fever, which is a
quence of intoxication, and as sometimes stomach in a state
dent of it, there is a peculiar mental dermulating and cor-
mania a potu, which is the subject of the es, is demanded,
l, and hot toddy,

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* Caldwell's Collection of Theses, &c.

This species of insanity is mostly to be met **with in** hal
tual confirmed drunkards, and in all cases, so far **as our**
perience extends, makes its appearance on the **subsidence**
the stimulating effects of the liquor. Two or three days
ways, and sometimes a much longer period elapses after
debauch before it makes its approach. Even the withdra
ing too suddenly the ordinary portion of drink, where
consumption is great, we have known, now and then, to
productive of the disease.

An attack is generally ushered in by a singular alterat
of countenance. The eyes are red, and have a very furi
expression, rolling constantly, with a wild glare, in differ
directions. The face is tumid and suffused—the carotid
temporal arteries pulsate strongly, and the patient compl
of violent head-ache.

At this period the skin is hot, the tongue parched,
pulse strong and full, though readily compressible, without
very distinct manifestation of fever. Early in the case th
is much gastric disorder—the bowels are obstinately con
pated, and soon come on violent vomitings, so that no f
can be retained, for even the drink—which an insatiable th
vehemently demands.

As the disease advances, there is an increase of nerv
tremors and muscular relaxation, to such an ext
indeed, that the person is rendered incapable of us
his limbs, having scarcely power to raise his hands,
when he attempts to move, his legs totter, and he fa
Yet, occasionally, paroxysms of immense strength occur,
which, in a moment of phrenzy, is so tremendously exer
as to require the united power of several men to resist
This state of excitement does not continue long, and is s
ceeded by

vociferates loudly, pulls his bed to pieces, throws about the chairs, attempts to make his escape, by even leaping out of a window, or remains quiet, betraying the utmost timidity, and complains of all the phantoms and terrible spectacles which such a distemperature can create, to harass or terrify.

Examinations *post mortem*, disclose to us precisely such phenomena, in this disease, as might be anticipated, from its cause and symptoms.

In all cases which we have inspected, the stomach was highly inflamed, and the liver variously diseased, sometimes with scirrhus, sometimes tuberculated, sometimes by an abscess, and sometimes recent inflammatory marks were added to these chronic conditions. .

The brain affords, for the most part, very conspicuous manifestations of its participating in the disease, as indeed we might expect, from its known connexion with the stomach.

We have seen it much inflamed or congested, and in other instances have met with serous effusions, and extravasations of blood, though never to any extent, as regards either of these fluids.

The thoracic contents seem to feel little of the immediate effects of the disease. As probably in all drunkards, water is found in the chest in various portions, and even dropsy of the pericardium is not an unusual occurrence.

Touching the treatment of this disease, some difference of opinion at present prevails. Examined superficially, we should be led by the state of the pulse and other indications, to the use of the depleting measures. But experience has taught us, that such appearances are deceptive, and, except under very peculiar circumstances, we are to adopt the directly opposite course.

The first step to be taken, is to place the stomach in a state to receive, and feel the impression of stimulating and cordial medicines. Little more, in mild cases, is demanded, than the neutral mixture with landanum, and hot toddy,

pretty freely given at short intervals, the patient being co
fined in a dark room, free from noise, and other annoyanc
to prevent his getting to sleep.

In some instances, however, there is such a total insu
ceptibility to the action of stimuli, that an immense qua
tity of opium and ardent spirits will make no impressio
or so slight a one as to be productive of scarcely a
advantage. Thirty or forty grains of opium, with a corre
pondent proportion of ardent spirits, we have known to
exhibited unavailingly in the twenty-four hours. To revi
gastric sensibility in the case, an emetic often proves high
beneficial. But to be effectual, the dose must be large, ev
twenty or thirty grains of tartarised antimony, and seve
drachms of ipecacuanha in divided portions, we have giv
to accomplish the purpose.

After copious evacuations of the stomach, in which t
bowels generally share, we may recur with the happiest e
fects to the opiate and brandy treatment, so urged as to i
duce tranquillity and sleep, which may be considered as a "*si*
qua non" to recovery. No instance of a cure, or even esse
tial improvement, so far as our memory serves us, has com
within our observation, where such a state of composu
was not induced. To this point, therefore, our attention i
steadily to be directed, and the remedies already designate
intrepidly employed till it is accomplished. Being onc
done, a manifest amelioration ensues, and the cure hencefor
ward proceeds rapidly and with certainty. In cases, how
ever, where the pulse is full, and not very compressible, an
which are marked unequivocally by undue determination
to the brain, we have sometimes ventured to bleed mode
rately, and very often under such circumstances to appl
cups and blisters to the head, and with utility.

But it sometimes happens, either from ill management, o
that we are not consulted in time, the case becomes typhoid
which is denoted by the dry *encrusted* tongue, hot skin, suf
fused countenance, wild eye, with contracted pupil, nervou
tremors, *subsultus tendinum*, low muttering delirium, &c.

The practice we have found most successful, at this stage or condition of the disease, consists of opium and camphor, the carbonate of ammonia alone, or what sometimes answers extremely well, with the musk julep,—the watery solution of assafœtida and strong brandy toddy.

The remedies, in every state of the disease, it may be perceived, with hardly an exception, are of a cordial nature, and which may be aided by a diet of a similar description, such as jellies with wine and spices, rich soups with Cayenne pepper, oysters, &c. &c.

Even when convalescence is established, the same system of living is to be continued, and now the malt liquors prove exceedingly grateful, and highly serviceable.

Nor can we for a time withdraw our medicines. Long after the subsidence of the more urgent symptoms, disordered stomach, morbid *vigilance*, and nervous tremors combine. To relieve these affections the carbonate of ammonia, camphor, Hoffman's liquor, assafœtida, garlic, hop tea, will be required, the one or the other being preferred, according to circumstances.

This is the plan of treating *mania a potu*, which we have found most successful. All the stimulating part of it has recently been claimed by a European writer, who arrogates to himself no small praise for having introduced it in place of the depletory measures formerly pursued in the case. But he has no title whatever to originality, it being, on the contrary, the established practice of this city, for upwards of half a century, and to the decided efficacy of which, we have the most abundant attestations.

Of late, however, the emetic practice suggested by Dr. Klapp, an ingenious physician of this place, has attracted no little attention. It would seem that he relies almost entirely on active vomiting, and states, as the result of ample experience, that it is incomparably the most effectual. Deciding on what we have seen, we should say that such a report is not quite warranted to this extent. Exactly, as in other cases of mania, where we have an insusceptibility to the action of remedies, which often happens, emetics in these temulent affections, are of the greatest utility, and Dr. Klapp deserves our acknowledgments for the extension of the practice to such cases. But sensibility being awakened, all is done which can be effected by emetics, and we must, to confirm the cure, revert to the stimulating articles as already described.

Even some degree of circumspection is required in the use of emetics. The cases of the disease must be properly discriminated, or they prove detrimental, and occasionally fatal. Exhibited, where there is much prostration of power, or, in other words, extreme *exhaustion*, the system will not re-act, and the patient irrecoverably sinks. Two cases of this kind came under our own notice, and several others have been reported to us.*

We have now given a condensed view of the contents of this very instructive Essay, interspersed with some remarks, the result of our own enquiries and observations. Yet, what we have described, is only one of an infinity of the afflictive consequences of the odious and detestable practice of *hard drinking*, whether we regard it as the source of moral debasement, or of corporeal infirmities.

The mythology of antiquity, often presents the finest moral, so shadowed, however, by allegory as not always to be discerned. We read of Prometheus having been chained to a rock, with his liver exposed to the perpetual lacerations of a vulture, as a penalty for an act of indiscretion against heaven. Considering that this organ is the seat of many of the excruciating affections, induced by the rash consumption of ardent spirits, we have here offered, whether meant or not, a most impressive admonition against a practice so subversive of the best hopes and interests of mankind. Nor is this all. To such an extent, indeed, does this multiplied mischief prevail, that, as has been remarked on another ocasion, "the emptying of Pandora's box, was the type only of what has since happened in the diffusion of rum, brandy, gin and whiskey, among the human family."

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* It is not altogether improbable that the quantity of tartarised antimony used in these cases might have had a poisonous effect, and hence it may be right, where large doses of an emetic are demanded, to substitute ipecacuanha.

As this is the case, it becomes the sacred and imperative duty of the physician to unite and confederate with the divine, the moralist, the economist, and all other good men, to restrain the consumption of these baneful articles—and as the first step in the scheme of reformation, to discountenance by the weight of his authority, the popular notion of their remedial efficacy, and to seize every fit opportunity to state truly and earnestly their pestilent operation.

Art. XV. *An Eulogium on the late* John Syng Dorsey, *M. D. Professor of Anatomy in the University of Pennsylvania. Delivered by appointment before the Medical Class, on the 1st of March,* 1819: By N. Chapman, M. D.

WE have met, my young friends, on a melancholy occasion. Death, always terrible in his visitations, has most awfully afflicted us.

Commencing with the very founder of our institution, he has swept off, in rapid succession, nearly all of the distinguished men who illustrated its character and maintained its ascendency.

In the short space of eight years, I have lost the whole of my preceptors in medicine,* and now, by an appointment too partially conferred, I am called to pronounce the eulogy of one of the most beloved of my colleagues. These sad events have converted the temple of science into a house of mourning, and every where within it, are heard the tones of lamentation and sorrow.

To me, this last dispensation has been peculiarly severe. As my friend and companion, mingling largely in my social recreations, and, more than any other, participating in my employments, the bereavement is irreparable. Even amidst the active and crowded scenes which have since engaged me, I have very often cast my eyes around, and found that I was solitary and alone.

Nor, perhaps, on any preceding ôccasion, was the public sensibility more strongly expressed. As soon as the unwelcome tidings transpired, the whole city was overcast with the gloom of a heavy calamity. The ordinary amusements, for a time, became suspended, general gaiety was eclipsed, and every countenance wore the aspect of grief and dismay.

To behold so much youth, and vigour, and usefulness, thus suddenly extinguished, was indeed a solemn admonition of the uncertainty of human existence, and the perishable tendencies of all our hopes and possessions.

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* Shippen, Woodhouse, Rush, Barton, and Wistar.

Death is very differently contemplated. An aged individual, however revered for his virtues, or valued for his services, sinking into the grave, is considered, for the most part, as a mere compliance with an inexorable law of nature, and the ultimate completion of an inevitable destiny. But, when one is cut down in the season of bloom and promise, we feel the intensity of the blow in the disappointment of our anticipations of future excellence, and can only be reconciled to it by the reflection, that though unintelligible to our limited perceptions, it is the work of divine wisdom, against which we must not murmur or complain.

By your conduct, in this instance, you evince the warmth of your sensibility, and have conciliated much cordiality of esteem. The spectacle which you now present, is exceedingly interesting and impressive. Like the people of antiquity, of whom the practice is recorded, of each one depositing some portion of the materials out of which to erect the tumulus over the remains of a favourite chieftain, with the same sense of affectionate attachment, you have convened, to render the tribute of respect to the memory of your benefactor and friend. To those to whom he was most dear, no species of homage can be more acceptable, and may we not indulge the hope, that even his immortal spirit is not indifferent to the proceedings of this day?

That the lives of literary and philosophical men are comparatively destitute of interest, has been commonly observed. Devoted more to meditation than enterprise, it is obvious that they must supply fewer incidents to excite curiosity, or command attention.

To a certain extent, the same remark is applicable to the members of the medical profession. But, though in this respect, we may not compete with those who lead the arms of their country to victory, or control the decisions of her councils by their eloquence or wisdom, surely he has some honest claims to notice, who so sedulously endeavoured to minister to the miseries of his species, and extend the limits of science, to spread the blessings of benevolence, and uphold the empire of truth and knowledge.

Entertaining this conviction, I shall proceed to trace th prominent circumstances of his short career, and perhaps, i may not be uninstructive to you to learn, what were th means by which a man, at an age when most of us begi only to attract observation, had already risen to great eminence, and if spared, would inevitably have reached the utmost heights of distinction.

Born in the city of Philadelphia, on the twenty-third o December, seventeen hundred and eighty-three, John Syn Dorsey was descended from ancestors, some of whom, especially on the maternal side, were advantageously know by their philosophical acquisitions, and general attachmen to liberal pursuits.

Of the early part of his life, I have been able to collec very little worthy of record. It appears that he received a excellent elementary and classical education at a school i this city, of the Society of Friends, then in high repute, an here manifested the same vivacity of genius, and quicknes in learning, with the mild and gracious dispositions fo which he was subsequently so conspicuous.

At the age of fifteen years he entered the office of his relative, the present Professor of Surgery,* and at this time our acquaintance commenced, which soon ripened into the closest intimacy, that continued without interruption or abatement, amidst the collision of interests, and opposition of views, so fatal to professional friendships.

Medicine, he cultivated with unusual ardour, and so successfully, that though by far the most juvenile member of the class, he had no superior, in the estimation either of his teachers, or fellow students. Of the force of his application and its results, a conception may be formed, when it is told, that while still very much within his minority, he was fully prepared for the highest medical honour of this university. In the spring of eighteen hundred and two, then in his nineteenth year, he graduated as a doctor in physic, having previously defended with ability, an inaugural dissertation "on the powers of the gastric liquor, as a solvent of the urinary calculi."

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* Dr Physick.

Criticism has rarely been exercised on publications of this nature. It may, however, be permitted me merely to remark, that the one to which I have alluded, exhibits some original views, illustrated and maintained by a set of pertinent and well conducted experiments.

Not long after receiving his degree, the yellow fever reappeared in this city, and prevailed so widely, that an hospital was opened for the accommodation exclusively of the sick with this disease, to which he was appointed resident physician. Of the manner in which he discharged the duties of this office of hazardous benevolence, it is difficult to speak too highly, so great was the value attached to his services.

Nor did he neglect the vast opportunities which his situation afforded of investigating the disease, and happily by his extensive dissections, elucidated some of the more intricate parts of its pathology, and aided in the establishment of a better system of practice. It may be safely affirmed, that no one was more correctly informed on the subject of this epidemic, and not a little which has appeared under the authority of other names, I am prepared to state, was derived from his observations and researches.

At the close of the same season, he proceeded to Europe, for the purpose of improving his medical knowledge, and liberalizing his views by a wider survey of the world. During his absence, he divided his time between the English and French metropolis, and diligently availed himself of the immense advantages, which in these respects, each city affords.

That his talents and acquisitions were duly appreciated abroad, we have ample evidence in the attention which was paid him, and in the very flattering notices he has since received in several of their writings.

In December, eighteen hundred and four, he returned home, and immediately entered on the practice of his profession. The reputation he brought with him, his amiable temper, and popular manners, his fidelity and attention, speedily introduced him into a large share of business.

From this period, professional honours were heaped on him with profusion. To the dispensary, the alms-house, and hospital, he was appointed surgeon, and in all our medical associations he held some elevated office. But there was reserved for him a still higher and more dignified station.

In eighteen hundred and seven, he was elected adjunct professor of surgery, where he continued till he succeeded to the chair of materia medica. Two courses of lectures he delivered on this subject, when a vacancy occurring in the chair of anatomy, by the lamented death of the venerable incumbent,* he was raised to that important professorship.

Considering himself now placed, for the first time, in the proper sphere for the exercise of his talents, and the gratification of a generous ambition, the appointment gave him much delight, and with ample preparation, he opened the session by one of the finest exhibitions of eloquence ever heard within these walls. But here ends his bright and prosperous career, and the expectations of success thus created, were not permitted to be realized.

Elevated to a position, above which he could hardly ascend, and surrounded by all that we most value, Providence seems to have selected him as an instance to teach a salutary lesson in the shortness of life, the insignificance of things transitory, and the importance of that eternity which absorbs all being and all time.

On the evening of the same day that he pronounced his introductory lecture, and while the praises of it still resounded, he was attacked with a fever of such vehemence, that in one short week it closed his existence, leaving to us only his enviable name, and inestimable example.

Than this, no event of the same sort, within the range of possibility, at the present time, could be more severely felt by the community, and I believe that there is not one of us who has bestowed any anxious thoughts on the state of our school, that will not confess his hopes clouded, and his spirit cast down by this heavy blow. The loss is indeed so great, that it cannot at once be replaced. Talents such as his are seldom possessed, and even if they could be met with, they would not immediately command that universal confidence which is necessary to constitute a great teacher or practitioner of medicine.

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* Dr. Wistar.

No high reputation or general influence can be gained by a single effort. They are the reward of long and unwearied exertion of sacrifices made, of ability proved by trials and difficulties. It was by a course of such arduous exertion, that he raised himself to the rank he held, and which no other individual can attain till he has ratified his claims, in the same way, to confidence and respect.

Dorsey was a man of no ordinary powers, and deservedly occupied a large space in the public eye. Naturally acute, vigorous, and discriminative, his mind was highly improved by education, and embellished by taste. Every department of medicine he had cultivated assiduously. But, it was for surgery he evinced a decided predilection, and in which he had the greatest proficiency.

As a science, he thoroughly studied it, and from the unequalled advantages he enjoyed, had become no less expert in the practice. Excepting one illustrious character, who has no rival, he was indisputably the most accomplished surgeon of our country, and this high praise is conceded to him on account of the number, the variety, the difficulty of his operations, and the skill, dexterity, and boldness with which they were performed.

Excellence in this province of his profession, he must have attained, under almost any circumstances, so many were his qualifications. Clear in his views, and of sound judgment, he had also great mechanical ingenuity, delicacy of touch, and promptness of decision; and hence, in conducting an operation, however new or complex, there was a tone and firmness of manner which always inspired confidence, and insured success.

er of medicine, his merits were great and uni- cknowledged. Early employed in this field of ex- on, his mind became perfectly disciplined, and the vari- us subjects to which it was directed, it developed without ltering or embarrassment. It was this quickness of appre- ension, and facility of execution, which caused him con- tantly to be resorted to in seasons of emergency, to supply e deficiencies produced by casualties in the school.

We have seen him on these occasions, in the same day, llustrate the operations of surgery, and deliver the details f the materia medica, demonstrate the minutiæ of anatomi- al structure, and expound the laws of the animal economy. Talent so flexible, and knowledge thus diversified, have arely been concentrated in one individual, and still more arely exhibited with such imposing effect.

It is remarked by a celebrated teacher,* at the conclu- ion of one of his introductory discourses, that he should ot attempt to give his class all which he knew, but so much only as he thought they were capable of comprehending, or might prove useful. The lectures of our friend were moulded on this principle, and it was one of the causes which conduced to his great success. Disdaining the parade of obsolete learning, the common resource of imposture in science, and those vain speculations which, like other vapours, darken and bewilder, he collected the most important matter, and closely condensed, he gave it in a mode plain, didactic, and impressive. Controlled by a rigorous judgment, he seldom indulged in declamation, or was seduced into wild and discursive sallies and digressions.

Never failing in whatever he engaged to teach, it was, however, in the demonstrative branches of medicine he particularly excelled. Not less by nature than study was he fitted for the undertaking. To exactness of knowledge, which he owed to a retentive memory, corroborated by the habit of ntense application in early life, he added a fluent elocution, an entire self possession, and a methodical and luminous mode of exposition.

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* Dr. William Hunter.

But in no situation did he appear to greater advantage than in the discussions of our Medical Society. Constituted of many of the more active, intelligent, and enterprising of the practitioners of the city, and of the members of the medical class, this institution is admirably adapted for the display of talent, and the reciprocation of professional information.

As a debater he never had a superior among us. The style of his speaking was peculiar and distinctive. Destitute of rhetorical pretensions, it had the character of warm and elevated conversation, and while it was sufficiently strong to cope with the most powerful, it was intelligible by its simplicity to the meanest capacity.

Equally adroit in attack or defence, the resources he exhibited in these contests, and especially when pressed by the weight of an adversary, were surprising, and often drew forth strong expressions of admiration and applause.

It has been objected to his speaking, that though always ingenious and forcible, it was occasionally loose and desultory. But this defect was visible only in those extempore effusions which escaped from him without premeditation or reflection, and proceeded, in a great measure, from the fecundity of his genius, and the copiousness of his matter. Teeming with ideas, and exuberant in facts, it was not always he could preserve his arrangement, or the chain of his reasoning, perspicuous and consecutive.

As a medical writer he is certainly entitled to be placed among the most prominent we have produced. To the periodical journals he contributed many valuable papers, and published the "Elements of Surgery" in two large octavo volumes, which is probably the very best work on the subject extant. Composed in a plain and unornamented style, it embraces within a narrow compass a digest of surgery, with all the recent improvements it has received in Europe and this country.

Dedicated as he was to his profession, he still did not neglect elegant literature or the liberal arts. On the contrary, he cultivated them with care, and found in the intervals of his leisure, that they smoothed the ruggedness of his severer studies, and afforded a refuge from the cares and irritations of business.

Between these chaste pursuits and the science of med cine there would seem to be a natural alliance. Every ag shows them to have been intimately associated, and in th beautiful mythology of antiquity the disciples of Esculapi and the votaries of the Muses have the same tutelary div nity.

Extraordinary as were the powers of his mind, they d not surpass the qualities of his heart. What was said b Burke of Fox, that he was born to be beloved, is striking applicable to our friend.

As much as any man whom I ever knew, was he calc lated to win attachments and disarm enmities. Cordia warm, generous, practising all the courtesies, and extendin every kindness in his intercourse with society, it was impo sible to approach him without being conciliated, and furth acquaintance served only to confirm the agreeable prepo sessions.

Frank and unreserved, there was nothing in his depor ment to inspire awe, or excite doubt or suspicion of h sincerity. No one, such was his habitual graciousness, however humble, was thrown at a distance, or rendered uncomfortable in his presence. Easy, cheerful, and good humou ed, he diffused these pleasant feelings around him, and enlivened every scene into which he entered. Mixing much i the circles of fashion, his manners, naturally urbane, wer highly polished, and his conversation, so various was hi intelligence, and such the pliancy of his address, woul amuse the gay, and instruct the illiterate, entertain the lear ed, and delight the grave and pious.

Yet, with this versatility of genius and diversity of pursuits, he overlooked no important concern, nor slighted an material duty. The review already presented suff circumstances, he could, in the most remarkable degree intermix amusement and business, without any serious encroachment, and preserve to a great extent, undisturbed, the order of systematized life.

As he lived so he died. Never shall I forget the truly impressive scene. When, by his peremptory command, the awful communication was made of his irrecoverable state, he was composed, firm, and resolute, confiding in the mercy, and resigned to the will of Heaven.

As a Christian, practising with more than ordinary punctuality the duties of his religion, death had to him fewer terrors. Emphatically and with fervour did he reiterate the expression of his confidence in the atonement of his Saviour, and the comfort which he derived from this source. What else indeed can sustain us at such a crisis? An audacious spirit, roused by the pomp and pride of war or a sense of duty or honour, will in the field affront death, and brave its consequences. But even he, in the gloomy chamber, and under the anguish of disease, where no such adventitious impulse exists, without this only support, will shudder at the idea of dissolution, and the destinies of eternity.*

As the foundation of all moral refinement, and as you regard your temporal and eternal interests, neglect not then your holy religion. Next to its own immediate functionaries, it is incumbent on you to nurture its spirit, and devote a decent attention to its external observances. Among other motives to do so, you will often be introduced into those vexed and troubled scenes in which, while endeavouring to heal the infirmities of the body, it will be not less your duty to offer the *medicina mentis*, the solace which it alone affords. At this conjuncture, he whom you may be invited to relieve, however much he might have previously confided in you, when the heart was light, and exulting in the plenitude of health, would shrink back, with instinctive horror, from the touch of the cold hand of scepticism.

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* Smith's Eulogium on Washington

But you also will require its consolations. No one can hope to escape the tempests of this life. There will be to you all, seasons of adversity and days of trial. Deep afflictions will sooner or later cluster around you, and you will have to mourn over the ashes of departed joys. Then will you learn the impotence of reason, and, though philosophy may enable you to endure, with becoming submission, these heavy dispensations, it is religion only which can assuage the agony of grief, and prove a lenitive in sickness and in sorrow.

These are the more prominent incidents, and such the reflections they suggest, of the life of him whose loss we deplore. It is now, in taking this sad leave, that we feel with renovated force the bereavement which in common we have sustained.

Let us, however, endeavour to repress our unavailing regrets, and forbear to indulge any rebellious discontents. What though his body lies covered with the sod of the valley, his soul has escaped to celestial regions, and partakes of the immortality of its God!

⁂ We shall publish Biographical Sketches of all the deceased Professors of the University of Pennsylvania, and shall be happy to receive and insert similar notices of other medical characters of our country.

MEDICAL AND PHILOSOPHICAL INTELLIGENCE.

Gluten, an Antidote for Corrosive Sublimate.—During the researches undertaken by Dr. Taddei on gluten, and on wheaten flour, he discovered that gluten had the property of acting on the oxide of mercury, and on corrosive sublimate. If it be mixed with either of these substances, it immediately loses its viscidity, becomes hard, and is not at all liable to putrefaction. Further, if flour be made into a paste with corrosive sublimate, it is impossible to separate the gluten and starch in the usual way. This effect induced Dr. Taddei to suppose, that in cases of poisoning by corrosive sublimate, wheaten flour and gluten would prove excellent antidotes to the poison. It was found by experiment, that wheaten flour and gluten, reduced corrosive sublimate to the state of calomel; and, also, that considerable quantities of a mixture of flour and gluten with corrosive sublimate, might be eaten by animals, without producing injury; thus, fourteen grains of sublimate have been given in less than twelve hours to rabbits and poultry, without injury, whereas a single grain was sufficient to produce death, when administered alone. A grain of the sublimate, required from twenty to twenty-five grains of fresh gluten, to become innoxious; when dry gluten was used, half this quantity was sufficient; but when wheaten flour was taken, from fifteen to eighteen denari, (500 or 600 grains) were required. Dr. Taddei recommends, that dried gluten be kept in the apothecaries' shops, and that it be administered when required, mixed with a little water.—*Giornale di Fisica*, 2. p. 375.

New Vegetable Alkalies.—Piperine.—M. Oersted announces the addition of two new vegetable alkalies, to those already known. One of these, to be called Piperine, is obtained from pepper, by digesting it in alkohol, and then water; the resin is precipitated, and the muriate of piperine remains in solution. The solution is to be evaporated for some time, and then decomposed by pure potash, which precipitates the new alkali.

Piperine is nearly insoluble in cold water, and only very slightly in boiling water. It dissolves in alkohol, and the solution has a greenish yellow colour, which, by the addition of m tric acid, is rendered a perfect green. Piperine is very acrid, with sulphuric and acetic acids, it forms salts, nearly insolubl in water. The muriate is moderately soluble. The capacity a saturation appears to be very small.

M. Forchammer has found a new alkali, in the fruit of th capsicum annuum. It is extremely acrid. It is more soluble i water, and has a greater capacity of saturation, than the oth vegetable alkalies. It forms a triple salt, with the protoxide o lead and muriatic acid, which is as acrid as the alkali itsel *Jour. de Phys.*

M. M. Desonnes and Clement, have been searching after th absolute zero, and are convinced that it is 266°.66 below th zero of the centigrade scale, or at — 446° F.

Preparation of Nitrate of Silver.—M. Brandenbourg ha invented an economical method of separating silver from copper, in the preparation of nitrate of silver, from the alloys of silver. He dissolves the alloy in nitric acid, and having evaporated the nitrates to dryness, places them in an iron spoon, and fuses them, until ebullition ceases: The fused mass is then poured upon an oiled slab. A small portion is tested for copper, by solution in water, filtration, and ammonia. If it is found by the blue tint, still to contain copper, the fusion is continued a few seconds longer. The mass is now nitrate of silver, and black oxide of copper. It is to be dissolved in water, evaporated and crystallized, and the pure nitrate of silver is obtained.

Veratine.—This alkali has been separated from the seeds of the *veratium sabadilla* by M. M. Pelletier and Caventou. This alkali is white and inodorous. It excites violent sneezing, and is a powerful emetic and poison. It is soluble in $\frac{1}{1000}$ parts of boiling water, and very soluble in ether.

Test for Copper.—If we drop into a newly prepared tincture of guaiacum wood a concentrated solution of salt of copper, the mixture instantly assumes a blue colour. This effect does not take place when the solution is very weak, as when there is not above half a grain of the salt to an ounce of water; but then, by the addition of a few drops of prussic acid, the blue colour is immediately developed of great purity and intensity. In want of the prussic acid, distilled laurel water, or that of plum or black cherry kernels, may be employed. This reaction succeeds where the proportion of salt to the fluid is not more than $\frac{1}{21000}$. In this proportion no other test, whether the prussiates of potash, soda, or ammonia, will develop the least indication of the presence of copper.—*Ann. Gen. des Sciences.*

Spontaneous Combustion.—About twenty-five pieces of cloth, each of which contained nearly thirty ells, were deposited upon wooden planks in a cellar at Lyons, on the 8th of July, 1815, in order to conceal them from the armies which at that time overran France. In the manufacture of the cloth twenty-five pounds of oil were used for a quintal of wool, and the cloth was quite greasy, each piece weighing from eighty to ninety pounds. The cellar had an opening to the north, which was carefully shut up with dung, and the door was concealed by bundles of vine-props, which freely admitted the air. On the morning of the 4th of August, an intolerable smell was *felt,* and the person who entered the cellar was surrounded by a thick smoke which he could not support. A short time afterwards he entered with a stable lantern in his hand, and he was astonished to perceive a shapeless glutinous mass, apparently in a state of putrefaction. He then removed the dung from the opening, and as soon as a circulation of air was established, the cloth took fire. In another corner of the cellar lay a heap of stuffs which were ungreased and prepared for the fuller, but they had suffered no change.—*Edinburgh Journal.*

Electricity.—A curious experiment in electricity is announced in the nineteenth number of the Journal of Science, &c. of the Royal Institution of Great Britain. By passing a strong charge from a battery through a piece of tin-foil placed between two insulated metallic knobs, the foil was perforated by two holes with their burs in opposite directions.

This phenomenon would appear to indicate the existence of two electrical fluids, moving in opposite directions.

Remedy for Bronchocele.—Dr. Coindet has announced, through the medium of the *Bibliotheque Universelle* for July 1820, some experiments with iodine in bronchocele. He has exhibited it in the form of a tincture composed of forty-eight grains of iodin and one ounce of alcohol, and in the form of a salt—forty-eigh grains of hydriodate of potash, dissolved in one ounce of wate The dose of either of these preparations is ten drops, three time per diem, gradually increased to twenty.

Rheumatism.—Dr. Williams of Ipswich has lately publishe some experiments with the seeds of the *colchicum autumnale* i chronic rheumatism, which appear to be very favourable to i character as a remedy in that painful and intractable disea It is given in the form of vinous tincture, dose ℨj *bis in die.* Th colchicum should be taken out of the ground in the month of A gust, and, to prevent its further progress in vegetation, it shoul be immediately cut in slices about the thickness of a dollar, an dried in a heat not exceeding 180 degrees of Fahr.

Antidote for Vegetable Poisons.—M. Drapiez has ascertained by numerous experiments, that the fruit of the *penillea cordifo lia* is a powerful antidote against vegetable poisons. The *rhu toxicodendron, hemlock,* and *nux vomica,* are mentioned as subject to the counteracting agency of this fruit when taken int the stomach, and the *manchenille* when applied to a wound.

Plantain Root.—Dr. Perrin has lately read to the Society o Natural Sciences observations on the febrifuge virtues of the plantain root (*plantago major, minor, et latifolia, Linn.*)

Test for Baryta and Strontia.—Baryta and strontia may readily be distinguished from each other by the following process: —Make a solution of the earth either in nitric, muriatic, o some other acid, which will form a soluble salt with it; add a solution of sulphate of soda in excess, filter, and test the clear liquid by subcarbonate of potash. If a precipitate falls, the earth was strontia, or baryta if the fluid remains clear.—*Journal of* and an external muscular covering. The former is very thin, and destitute of fibres. It is thrown into folds in the collapsed state, and upon its surface are numerous orifices of glands. The latter is made up of short interwoven fibres, forming fasciculi, united by an elastic substance of the consistence of muscus. These facts shew, in the author's opinion, the fallacy of the common opinion, that the lining of the urethra consists of circular contractile fibres, and thus throws a new light on stricture; a spasmodic stricture being caused by a contraction of a small portion of the longitudinal muscular fibres, while the others are in a state of relaxation; and a permanent stricture, by the exudation of coagulable lymph, in consequence of inflammation between the fasciculæ of muscular fibres, and upon the internal membrane.

After briefly noticing what is already known respecting the stricture of the corpus spongiosum and corpora cavernosa, the author proceeds to relate Mr. Bauer's observations on these parts. The cellular structure of the corpora cavernosa is made up of very thin membranous plates, very elastic, and so connected as to form a trellis-work, the edge of which is attached to the elastic ligamentous substance which surrounds them, and which forms the septum between them. The structure of the corpus spongiosum was stated to be similar to that of the corpora cavernosa, except that the parts are formed on a smaller scale, and that there are no muscular fibres in its ligamentous elastic covering. These observations were illustrated by several beautiful drawings made by Mr. Bauer.—*Lond. Med. and Phys. Journal.*

Results of Anatomical Inquiries on Tetanus, &c. &c. By Dr. Saunders, Lecturer on the Practice of Physic, Edinburgh.

1. If any muscle, voluntary, or involuntary, is affected with spasm, and, during this affection, the person dies, on examination it is found, that the nerves, which supply the spasmed muscle, are covered with turgid red vessels at their visible origins, or where they appear to set off from the brain, medulla oblongata, or spinal marrow.

This turgescence, and the effects of turgescence, are in the ratio of the degree and duration conjointly of the spasm or convulsion.

The turgid vessels in every obstinate and severe case, may

be traced into the substance of the spinal marrow, by the sid
of the striæ, which seem to be the continuations of the nerv
filaments; as also along the nervous cords, through the
sheaths formed in the dura mater.

The position of the body after death has no appreciable i
fluence on these appearances: they are observed anteriorly
posteriorly, in the loins, thorax, cervix, or within the sku
bearing strict relation to the parts which have evinced spasm
dic action.

But the nerves serving the muscles which have not labour
under spasm or convulsion, are free from turgid vessels.

2. If the tetanic affection is confined to the jaw, certain ner
arising from the tuber annulare and medulla oblongata, are fou
in the state above described.

But if the tetanic affection involves the whole inferior ext
mities, and the trunk of the body, as well as the jaws, then t
origins of the nerves from the *tuber annulare* to the *cauda equi*
are covered with turgid red vessels.

In short, the nerves exhibiting such turgescence at their o
gins, correspond in number and situation with the muscles whi
have exhibited inordinate contraction. I have conducted th
investigation for about sixteen years, and have not met with o
exception.

Some are of opinion, that I maintain, that the spinal marro
its nerves and membranes, are always affected with turgid ve
sels in tetanus: this is incorrect. I have examined cases of tri
mus, in which the spinal marrow, its membranes, and nerve
were almost entirely sound, from the atlas to the lumbar vert
bræ; in these instances, however, not only the origins of th
nerves at the medulla oblongata, but the medulla itself, was i
closed with a close net-work of turgid red vessels.

There are many other appearances within the cranium an
spinal canal more or less connected with spasms and convu
sions; but those, which I have here related, are uniform, and a

If you deem this communication deserving, let it have a place in the Journal.—*Edinburgh Medical and Surgical Journal.*

Further Remarks on the Case of Ligature of the Arteria Innominata. By VALENTINE MOTT, M. D.*

In my first communication of this case in the New York Hospital Register, for 1818, it is stated, page 50, that "*the subclavian artery, internally and externally to the disease, was pervious.*" To this it may now be added, that where this artery opens into the ulcer, left from the wound of the operation, it appears not only pervious, but of the natural size, and the coats free from any diseased appearance. Externally, towards the axilla, the artery is somewhat enlarged in diameter, but exhibits no appearance of disorganization of its coats either externally or internally. About an inch from the ulcer, or just as the artery has passed between the scaleni muscles, there is an irregularly shaped eliptical opening upon its upper side, large enough to receive the extremity of the fore finger. The edges of this opening are jagged and uneven, and the surface of the artery, internally, is of a brownish yellow colour, to the extent of half an inch on the inside of the opening, and more than an inch towards the axilla. The internal coat of the artery has a rugous or puckered appearance; separated a little from the muscular coat, very friable, and evidently in a degenerated state. This opening of the artery communicates directly with the anterior extremity of the sac, which contains coagula, and upon removing these the surface of the sac is seen puckered, or thrown into a great number of little folds, giving it, at first sight, the appearance of containing a number of lobes. This account is taken from the morbid parts before me, and the preparation has been seen and examined by Dr. Post, Dr. Hosack, Dr. Stevens, Dr. Watts, and others; who have authorized me to state, that they are satisfied as to the nature of the case.

New York, 9th mo. 29th, 1820.

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* Vide Eclectic Repertory, January, 1819. Vol. ix. No. 1

LIST OF NEW PUBLICATIONS.

[From the Journal of Science and the Arts.]

A Treatise on the Inflammation of the Mucous Membrane o the Lungs. By Charles Hastings, 8vo. 10s. 6*d*.

An Address to Persons afflicted with Deafness. By W. Wright, 4s.

A Synopsis of the various kinds of Difficult Parturition, wit Practical Remarks on the Management of Labours. By S Merriman, 12s. boards.

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A Toxicological Chart, in which are exhibited at one view, the Symptoms, Treatment, and Modes of Detecting the Various Poisons. By a Member of the Royal College of Surgeons in London. 2s. 6*d*.

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THE

PHILADELPHIA JOURNAL

OF THE

MEDICAL AND PHYSICAL SCIENCES.

ART. I. *On Reasoning in Medicine.*

By JAMES MACLURG, M. D. &c.

> C'est une erreur de croire, q'une experience aveugle et une habitude mechanique, tiennent lieu de principes surs, et de maximes fondées sur un solide raisonnement——
>
> *Traduct. d'un discours sur la commerce, par M. le Marquis* BECCARIA.

THERE is no art, unless this term can be applied to religion, which seems to require in its professors such a perfect harmony of opinion, as the art of medicine. For there is none in which error is of so much consequence, or where a fluctuation of the mind between opposite sentiments is attended with such anxious distress. Yet medicine has its schisms; and the sages of our art, while they appear devoted to Truth, forget her peculiar caprice; that, though she loves to be pursued with ardour, she must still be addressed with indifference.

The keenest shafts of ridicule have frequently been levelled at this trait in the medical character. Our singular gravity and solemnity placed us, as it were, in opposition to the free, the gay, and the witty; and they eagerly seized a circumstance, which seemed to reduce this extraordinary figure to the common size; and to show, under a face of great wisdom, much ignorance and uncertainty.

have replied, that physic, like the other sci-
ded on experience, is continually progressive,
therefore subject to perpetual change; so that, without
e suggestions of vanity, we may sometimes be tempted to
er from our venerable ancestors. And it might have been
oined, that physicians, notwithstanding some curious
ternal differences, which would deceive perhaps an inex-
rienced Houynhnm, are yet no better than mere Yahoos.
t it seems to be agreed, among the philosophers who have
amined this species with the greatest accuracy, that *to*
sociate is not more a principle of human nature than *to se-*
rate. For, though men have been allured by the prospect
advantage, or compelled by force, to unite themselves in
st bodies; yet it was always observed, that where the
litical bands which held them together were weak, and
y could follow their natural tendency, these huge piles
on broke into a number of pieces. And in all the coun-
ries where the arts of policy had scarcely taken root, the
inhabitants have been found divided into numerous tribes, inflamed against each other with a mutual animosity. In like manner, proceed these philosophers, we have seen a set of religious notions overrun half the globe: but the torrent soon split into a multitude of streams; and numerous sects arose, who persecuted each other with an unrelenting fury. It is not attraction alone which governs the material world: the laws of repulsion have also their influence; and it looks as if they had both their analogous principles in the moral system.

But these internal factions, which range us under the banners of so many leaders, are equally opposite to a set of men who will acknowledge the authority of neither. The sticklers for different systems, however averse to each other, unite like our domestic parties in politicks against a common enemy. It is hard to say when this quarrel began between empirics and dogmatists, or when it will end. It seems to be as ancient as the origin of reasoning in physic, which could not be long after the beginning of observat itself; nor will it cease, most likely, until our philosophy has acquired perfection; an æra still more remote.

Every step of experience approaches towards system; since, by discovering the natural relation of things, it prepares us for establishing principles. And theory, or the application of these principles in explaining the phenomena around us, is the favourite process of the human mind. So delighted are we with it, that we cannot wait the necessary preparation; and our eagerness would fain anticipate those principles which yet remain to be discovered. A vapoury gleam of fancy is mistaken for the ray of science; and we pursue it through all the treacherous ways of hypothesis.

For, though things, being related in nature, present themselves in a certain order to the view of the observer, he does not immediately discover that order which is invariable. Amidst the variety of circumstances which surround any fact, to seize that whereon it necessarily depends, requires a close attention, and commonly a varied experience. But the mind will follow in the mean time its propensity; and theories are produced, and destroyed, by successive observation.

System has so often fallen under the stroke of experience, that we have been led to imagine a natural enmity between them; which, by an easy transition, is extended to their several patrons. A theorist and an empiric are, in medicine, terms of mutual reproach, and as much the badges of two opposite parties, as those of whig and tory used to be in politics.

Amidst the recriminations of both sides, it seems to be acknowledged, that improvement in our science has ever been the fruit of growing experience; and that a multitude of errors have been sanctified by the authority of systems. And it is imagined that we go but a little farther, when we affirm that these are useless, and even dangerous; that, far from aiding the progress of medicine, they lie in its way, or mislead its steps.

But their danger, in the present state of physic, is surely imaginary. If they inflict a wound, they bring along with them its remedy.

> ————————Qua cuspide vulnus
> Sensit, et hac ipsa cuspide sensit opem.

The continual succession of systems prevents their acquiring an influence injurious to philosophic liberty. Their opposition has had the same effect in science, that the quarrels of contending popes produced in religion. It banished the notion of infallibility, which is so apt to be fostered by the blind veneration of a sect. Those gigantic forms of superstition, and authority, that were encountered with difficulty, and even danger, by the first literary knights-errant, no longer bar the avenues to science. Every subject is discussed by a free dispute; and the repeated appeals to reason and common sense, prove that we acknowledge no authority which is not derived from them.

This notion of a dangerous influence in systematics, may suit a supine, an ignorant, and a superstitious age ; but has no relation to the present, which is so remarkable for its free and active spirit of inquiry.

But shall we grant that they are useless; and is it credible that the industry of these gentlemen should be so active to no sort of purpose? A system cannot be built without materials; and these can be furnished only by observation. Empirics claim this field as their own: but it is evident that the dogmatic also must cultivate; and he will cultivate it with greater ardour, and probably with more success. It is passion which excites the talents, as well as virtues, of the soul to their most admired efforts. We are astonished at his unwearied industry, whose curious eye seeks every where support for a favourite doctrine. And it is a certain fact, that the sphere of observation has been farther extended by dogmatics, than by the men who pretend to give it so much merit.

Yet it is not a mere acquaintance with particulars in detail; but a knowledge of their relations, and especially of that important one by which they are fitted to produce one another, that we are chiefly interested in acquiring. And this knowledge should naturally result from the full and large comparison, which must precede every attempt at arrangement. Observations which had long remained solitary and neglected, are brought together, and acquire an useful importance. Some steps are made in forming principles; and the mass of confused experience begins to put on regularity and distinctness.

And though our associations should not be exactly agreeable to the order of nature, yet they have some advantage, as they assist the memory; which with difficulty retains a set of unconnected facts. Without the aid of system, the informations of experience must slide imperceptibly down the tide of oblivion; for no one could collect any number of them; and he would be still less able to deliver them to others. On this account, system is necessary in physic, as it is in natural history. In the former, indeed, it pretends to explain the animal operations, and to show us how we may influence them; and is founded, therefore, on the relation of cause and effect. As this is no object of the senses, but an inference of our fallible reason, it is extremely apt to be mistaken. The attempts of the medical systematic will frequently miscarry; yet still it must be acknowledged that his errors are readily detected. His course is regular, and may easily be traced.

But while he feels a warm affection for the child of his own fancy, he pursues, with severest scrutiny, the schemes of every rival theorist. He expunges what was absolutely false; and limits what an enthusiasm, similar to his own, had extended beyond its proper bounds. A succeeding dogmatist shows him the same impartiality; and lops, with as little tenderness, the luxuriant growth of an over-heated imagination. One is ready to believe that this ardour, so well adapted to invention and new combinations, corrected by that *sang-froid* which a man possesses in examining the works of another, though a perpetual source of private dissention, must tend, at least, to the general good, and quicken the progress of science.

And accordingly we observe, that as the rise and struggle lenote firmness and health in our excellent poli- stitution, so the perpetual growth and contention of ems is a mark of vigour in the state of medical know- lge. When Europe was sunk in ignorance, and every sort improvement languished, Galen tyrannized for ages in e schools of physic. The first dawning of the rays of sci- nce

> —— Ubi pulsam Hyemem Sol aureus egit
> Sub terras, cœlumque æstivâ luce reclusit.

seemed to restore some activity to the torpid minds of men, and disclosed the buds of another system. But, since that iod, they have been germinating continually; and their y seems to be proportionably accelerated. Cullen ac- quired the greatest authority in modern times; but his own pupils have thrown it off, and introduced innovations of the utmost importance.

This prolific birth, as well as transitory existence of systems, are evidently connected with the progress of experiment and observation. They remind us of the ancient fable of Saturn and his offspring, while they appear to devour the theories to which they gave origin. Yet it is unjust to charge the surest faculties of our nature with these unnatural, and short-lived productions. The information of the senses is right and true; though the opinions which we form in consequence are often erroneous. It is these opinions, the precipitate combinations of that busy organ the fancy, which experience dissipates in its course, while it unveils the more beautiful order of nature. And if, in its progress through the series of connected being, it should ever reach the ultimate point, our system will be that of the world, and represent exactly the disposition of things, which flowed originally from Almighty Will. In any other point of this progress, our arrangements must evidently be deficient; and, from a strong propensity of the mind to carry things to perfection, and to give them the appearance of a whole, we shall be continually supplying something from the imagination. This, I confess, should prevent our being wedded a system: yet we may certainly treat it as a mistress;

brace it with ardour at present, and discard it whenever we are disgusted with its defects, or attracted by the superior qualities of another. For, the best system is only the expression of our degree of knowledge, at the time of its production; and their continual change, while it discovers their weakness, is a mark of their growth and tendency toward perfection.

But, as these systems are the offspring of observation, so do they guide and assist its steps. They present a mixed prospect, where some parts are fully illumined by experience; while others are only faintly distinguished; or buried, perhaps, in a total obscurity. This is the chaos of science; and it is the business of the human mind, while it traces with wonder the operations of the divine, to diffuse over it the beauty of order. And though a difference of disposition, or more frequently of situation, will lead one to observation, and another to arrangement; their labours, far from being opposite, are mutually necessary. The quarrel between empirics and dogmatics is unnatural. As well might the senses quarrel with the intellect, and each pretend to exercise their function separately. Every one sees that they are useful only as combined; and that their excellency consists in their aiding, and correcting each other.

To exclaim against every kind of system, has been reckoned a proof of earnest zeal for observation. Yet what is it, really, but to declare, that we must rest stupid, and indifferent spectators of the train of facts continually passing before us? For, in viewing them with any care, we soon perceive that some in the succession are intimately related. The attention excited pursues these through a variety of cases, proportioned to the extent of our sphere of observation. And a more than natural coldness must guard the temper, which escapes the effect of an emotion that hurries the mind into theory. A mere observer is a very uncommon creature; and we are rarely presented with facts unmixed with speculative notions. More frequent is that precipitation, which from observing the connexion of certain

facts, in a few instances, pronounces it to be constant
necessary. A perfect indifference about the matter may
cure us from this error; but surely a fever of the mi
which tends, though irregularly, to use and improvem
is preferable to a fatal torpor, suspending its most imp
tant operations. The error will be corrected by the p
gress of experience; and frequently, in an inquisitive a
the having formed a false opinion will lead to the discove
of the truth; because it directs the attention, that source
all discoveries, to a particular point.

The number of absurd theories should not, therefore,
spire us with an antipathy to the term; nor must a pa
terror of them banish physicians from the sacred temple
philosophy. To be hurt with the imperfect and puerile co
mencements of reasoning in physic, and to relinquish t
hopes of a rational theory, is to be offended at the childi
prattle of infancy, and to expect nothing better from a m
turer age. That Being, who formed the mind of man pec
liarly capable of improvement, and though he fixed the
mits of existence, and of the acquisition of knowledge
the individual, appointed no such boundaries in the specie
has implanted in our breasts, together with curiosity,
fondness of system; and it is justified by the relations
things. Experience, while it discovers these relations, give
occasion to the exercise and display of that passion. I
therefore, we are determined to reject all system in medi
cine, let us oppose every obstacle to the natural progress
knowledge. We may begin with rejecting those science
anatomy, chemistry, and every branch of physics, whic
delude us continually with the hopes of penetrating int
the secrets of the economy. Impenetrable dulness shoul
henceforth be as necessary an appendage to the professio

barren and little improved understanding, let him deny the use of theory. We believe, most sincerely, that he is ignorant of its proper use; and when he finds that all the profits of medicine can be reaped without such a troublesome instrument, he may really think it unnecessary,

Gestit enim Nummum in loculos demittere, posthoc Securus.——

Yet there are some, it is said, to whom the progress of our art is less indifferent, that have adopted the same notion. Diseases, they apprehend, like the other productions of nature, are known and distinguished by their sensible properties; and their cure is only taught by experience. What occasion then for theory, which has so often deceived us? The knowledge of diseases, and of their remedies, can only be acquired by an assiduous attention to practice; and an hospital is the only useful school of physic.

Nor shall we deny, that a certain assemblage of symptoms gives to diseases their sensible form. It is the arrangement of these forms, according to their relations and differences, which makes the business of nosologists. They adopted the method, that had appeared so successful in the distribution of animals and vegetables. But these are multiplied continually in the same form; and Nature has drawn a line between their several species, which effectually prevents their confusion.

What are the species of diseases? A fever, a diarrhœa, a jaundice, an epilepsy, are apprehended to be all of a distinct nature; yet they sometimes concur in the formation of one disease. From the different constitution of the machine in individuals, in the conspiration of its parts, and the influence of those external powers to which it is continually exposed, its disorders appear under a great variety of forms. To determine what symptoms, in any combination, are essential, and mark the specific nature of the disease, must frequently be a difficult task. It will certainly require that symptoms be traced to their origin; that the historian should turn philosopher; and we may probably differ as much in our nosological arrangement, as in our reasoning on the phenomena of diseases.

n e mineral kingdom, the method of the botanists was nd insufficient; and soon yielded to one erected on a mical investigation. For here, the limits of combina- n are so wide, and the consequent productions so vari- , that our attention was confounded, when we directed only to their external and obvious qualities. An inquiry to their composition, has furnished us with an useful dis- ribution of them, according to essential differences.

The symptoms of diseases may be considered as a lan- age in which Nature addresses us; as signs whose mean- g we are to investigate. We must not be contented with nerely observing them, any more than with simply re- king the phenomena of the inanimate world; but should avour to trace them to those laws of the economy on which they depend. The improvement of anatomy and emistry, the dissection of morbid bodies, and experiments living animals, have certainly carried us some way in this inquiry; and by settling the theory of diseases, have contributed to mend, or confirm our practice.

If this practice had attained perfection; if every disease was defined, and the best method of treating it exactly determined; there would, indeed, be no occasion for theory. Yet, though we could suppose that men, conducted by their mere feelings, had reached this pitch of excellence, in the most difficult of human arts; we must still have commended that curiosity, which, not contented with simple imitation, should attempt to draw the veil; and by discovering the reasons of this successful proceeding, establish it on the best and surest ground. But if we modestly acknowledge a practice, which is so various, to be still imperfect; and if diseases will occur either entirely new, or so altered that the mere light of experience cannot guide us in their treatment; we shall not readily give up those principles, which must conduct us where example fails. Yet it cannot be denied, that successful modes of practice have grown up, we know not from what origin; or have been introduced by the most ignorant empirics. But have no very useful machines been invented by people, who we ignorant of the principles by which they were governed? And does it follow that a knowledge of the laws of mechanics is of no use to the artist?

But theory carries the mind through the whole circle of sciences, in its search after analogous phenomena to explain those of life; and in its inquiry into the powers which influence the animal operations. And some have imagined that this diffusion of the attention is unfavourable to the progress of medicine. Their idea seems to have been taken from a view of those arts, which are brought to perfection by means of their subdivision into the minutest branches. Nor do they find any absurd analogy in supposing, that as our shoes are much better, since each manœuvre in their structure has employed the whole skill and address of a reasonable creature; so the management of diseases might be rendered much more dexterous, by limiting the attention of the physician. And, in consequence of this doctrine, they have proposed to make every disease the care of a particular person; just as wisely as ancient superstition believed that every tree of the grove required its tutelary deity.

The mere arts may be improved by repeated subdivisions but those which partake of the nature of sciences require a different method of cultivation. It is the remark of a celebrated historian, that the art of war, for some centuries the only object of attention to all the gentry of Europe, acquired little improvement in the hands of men, who despised every other sort of knowledge. A personal dexterity in the management of their arms was common enough; but an extensive system of operations required an acquaintance with principles, which mere habit could never teach.

The reader, I hope, will not find any thing whimsical in this comparison of arts, which have equally the appellation of murderous; nor be led to fancy that a dogmatic and an empiric differ like a mareschal Turenne and a savage warrior; chiefly because the former pursues more scientifically his destructive schemes. A less exceptionable illustration may be found in the progress of an art very closely connected with medicine.

While the knowledge of chemists was confined to th operations within the laboratory, their art, though destin to throw a light on so many parts of nature, was full mystery and confusion. The variety of phenomena, mult plied by every day's experience, inspired an admiratio that arose to enthusiasm, and gave probability to their al surdest fancies. They did not attempt to explain these ph nomena by showing their relation to what happened i other parts of nature; for they were acquainted with n other part. But the mind will always be striving to gen ralize its notions; and system is as natural to mankin as invention. Certain facts made a more forcible impres sion on them: to these they referred every phenomeno which had any appearance of analogy; and they erecte them into governing principles of the world. They behel every where the operation of acids, alkalies, nitre, and sul phur; and corrupted the theory of medicine. The eye of philosopher pierced this cloud, which the manœuvres of thousand artists seemed only to render more thick an obscure. It was indeed a philosopher; one who looke through nature, and discovered the springs of that variety of movements which form the changing scene of our world It was Sir Isaac Newton!

The sciences, like the graces, march hand in hand; an nothing would be more vain than an attempt to pursue an one of them separately. Yet it will be objected, perhaps that the animal economy has so little in common with th rest of nature, that its facts can never be explained from any view of the phenomena of the inanimate world.

And indeed, the first movers in our machine seem t have little analogy with other natural powers; and to b past finding out. But is it certain that they will always ap

Franklins, extend still farther the boundaries of science? We can say nothing concerning the unknown parts of nature, except from the notion of a consistency in the whole plan. And many philosophers, observing that the various appearances of the universe are traced to a few ultimate principles, which seem to be themselves related, were tempted to imagine a common source of all the activity observed in the material world.

But, however distinct our machine may be in its kind, it makes a part of the general system; and is subjected to the action of all those powers by which it is surrounded. And though, perhaps, no animal operation can be understood from mere mechanical, or chemical principles; yet it does not follow, that these have not really their influence. For the operations are the result of the mutual action of several powers; and generally, in the combination, some occur that are unknown, and whose effect we cannot value. Our theory, therefore, is rather incomplete, than false; and instead of being discouraged from its use, we should be stimulated to advance it gradually to perfection.

And though the animal system were solitary, and unconnected with the rest of nature, a general knowledge would be necessary to a physician. For it must still be admitted that medicine, like the other sciences founded on experience, is capable of acquiring from induction principles of its own. And we cannot better learn the method of giving it this improvement, than by tracing the progress of the mind, in collecting from observation the laws of nature.

The knowledge of a certain routine of hospital, or other practice, is not sufficient therefore to prepare one for the exercise of our art; unless we would have it to be stationary. And there is even a probability, that a man may be continually conversant with diseases, without getting acquainted with them; as one may grow up in his own country, without knowing its characteristic manners. Objects must be seen in different lights, that our attention may be excited to observe them. Mere curiosity, though a lively, is a transitory passion, and is satisfied with superficial views.

d when nce it is satisfied, the same thing may return housand times, without making any impression. But en it is compared with similar, or contrasted with oppo- i acquires, as it were, a fresh novelty; and our s powerfully roused as by the first emotion of iosity. nd here lies a principal advantage of system, d on experience, to the man who applies himself to vation. Each fact, that presents itself, being viewed lation to a number of others, all its circumstances nspicuous: and every thing being connected with else makes a deeper impression, and is longer ned by the memory.

if there is truth in what has been said, nothing can re absurd than the vulgar notion of a certain quackish xterity in medicine; which leads men to confide in ignorance, supported by its constant companion, presumption. This notion, which tends to degrade physic from the rank of sciences, is not even applicable to surgery; though it is confessedly more purely an art than physic. The celebrated Petit, of Paris, constantly declares in his lectures, that the separation of physic and surgery is unnatural; and that it would be for the advantage of both, again to unite them. Yet, as far as simple dexterity is concerned, the chirurgical art has certainly been improved by the separation; and when this is carried farther, and surgery splits into several branches, the dexterity continues to increase. We may appeal to those fine manœuvres, for which the oculists, aurists, and dentists of the present times are so much celebrated. But it is not from the dexterity which we see displayed in artificial productions, that we are to judge of what is required in operations on the animal machine. In the former, it is art which does every thing; but in the latter, all the nicer changes are performed by nature, who completes and finishes what the medical artist rudely prepares. It is not, therefore, a mere dexterity which we require in a surgeon. We expect that he should be acquainted with the powers of the machine, and be able to tell "*quid ferat, atque faciat natura.*" And thus, his art so strictly connected with medicine, as to justify the remark of Petit; and though it may be useful to practise them separately, they should certainly be studied together.

But it must be remembered, that when we take up the defence of system, it is merely in a general view. Nor should a vindication of the privilege of reasoning in medicine, be extended to all those modes of exercising it, that have frequently proceeded to the length of licentious abuse. A description of the variety of opinions which they have produced, would look like an attempt to give a history of the caprices of the human mind. Some of them were scarcely born, before they were buried in oblivion,

> Ostendunt Terris hunc tantum fata, &c.
> Fate show'd them--but so quickly dropt the scene,
> 'Twas straight forgot that they had ever been.

While others, winged with stronger fancy, mounted above the weak view of their age; and

> Through a cloud
> Drawn round about them like a radiant shrine,*

they shone, for a time, the object of universal admiration. But they were mere meteors of the night; and as soon as we were in a condition to examine them, they disappeared. Perhaps, many of those theories, which at present look so firm and durable, are destined to a similar fate. One would think that, like the bodies of the sensible world, they were continually decaying, and continually reproduced in new forms.

Nor are we surprised at this account, when we reflect, that a system is an attempt to explain the phenomena of the living body in health, and in disease; and that to make it complete, a perfect knowledge of all the powers of the economy, and of their relation to one another, as well as to external agents, is absolutely necessary. But no one imagines that medical science, even in our enlightened age, has reached this point of perfection: and all are agreed that our predecessors, the nearer we approach to the origin of experimental philosophy in Europe, were plunged in a

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* Milton

still deeper obscurity. Much, therefore, of every syste
though not of every one indifferently, must be merely co
jectural. But this is the region of fancy, fruitful in prej
dice and error; where a variety of circumstances, even a
cident itself, have frequently a greater influence than reaso
and common sense. And if such is the temper of mankin
that there is scarcely an absurdity which authority canno
set up, and habit establish, they should treat with some i
dulgence the common frailty,

> ———— Æquum est
> Peccatis veniam poscentem reddere rursus.

By attending to the animal operations, we discover fac
of a very different kind; some of them have an evide
analogy with the phenomena of the inanimate world, an
seeming to be explicable from known principles; whil
others appear to be totally distinct in their nature, and caus
The former are considered as the effects of some of thos
powers, which we distinguish by the general appellations o
chemical, and mechanical; while the latter are said to de
pend on an energy peculiar to animals. And thus we arrange
in separate classes, the informations of experience concerning the animal machine; and each of them is to furnish u
with principles for explaining its phenomena.

But nothing can be more different than the notion of th
limits of these systems, and of their importance in the economy, in the minds of different theorists. Like the undetermined bounds of the powers of our political constitution
they vary continually with the prejudices of the individual
People are extremely apt, from contemplating the operatio
of certain energies in a variety of instances, to grow possessed with the idea of their consequence, and to imagin
their influence where it is not evident. Physicians, from th
circumstances which direct our habits of thinking, hav
become chemists, or mechanics; and applied their favourit
principles on every occasion. Their systems grew up in th
retirement of the closet; where men frequently indulg
themselves in speculations, which they never think of ap
plying to the busy scenes of life.

But they who delivered themselves more to observation, and the study of the animal machine, perceived something in all its affections, which distinguished them from every other natural operation. And when they saw that analogies from the inanimate world were inadequate to the explanation of the phenomena, they began to think that they should be altogether rejected.

In vitium ducit culpæ fuga, &c.

The animal operations, they believed, must depend on the energy of that being, which resides only in animals; uninfluenced by mechanical or chemical laws, since we get our idea of it, by denying it every property of matter. But reason informs us that it is endued with intelligence, and that self preservation is the powerful principle of its action. They naturally therefore fell into the doctrine of final causes; and conceived that the only way of explaining any fact in the history of the economy, was to shew that it was useful, or necessary.

And thus have arisen those systems of physic, which, with the titles of Mechanical, Chemical, and Stahlian, have made the greatest figure in modern times. They are liable to the common objections against systems in general. They owe their premature birth to our eager desire of explaining every thing; which cannot wait until the progress of observation has ascertained all the principles that are necessary to a perfect theory. Their authors are led away by an excessive fondness of simplicity, which they consider, perhaps, as ornamental to their ideal fabric. They pretend to explain all the operations in the animal body, from the view of one set of causes; just as they trace all the various affections of the soul to one common principle.

The animal machine, though it is connected with the other parts of nature, is distinguished from them; and, while it acknowledges the influence of the common laws of matter, is governed by a principle peculiar to life. Nor do I mean, by the vital principle, the soul of the Stahlians; but that energy which discovers itself particularly in muscular contraction; and is evidently different from elasticity, or any other known power in nature. Its exertions do not depend merely on the intelligent mind; since we observe them in muscles cut out from the body, and excited to action by heat, and other irritations. But, though we perceive its action independent of intelligence, it would be difficult to imagine a mind, such as ours, unconnected with this power. Unless we could conceive a mind without ideas, or ideas without sensations; pleasure and pain without feeling, or the passions relative to these without their objects; and, finally, volition without the capacity of action. By muscular motion alone are we led to the belief of an immaterial principle in animals. And if this was not the only, as well as sufficient ground of our belief, vegetables, nay minerals, for ought we know, might be possessed of souls full of differently combined ideas, and subject to all the variety of passions; since without this power, and machines like muscles which it might set in motion, they could never discover themselves to our senses. Every man can perceive in his companion the strong emotions which agitate his soul; especially where the natural behaviour is not confined by the rules of politeness, or rendered perplexing by the complicated influence of several passions. Yet he sees only the effects of certain changes in the muscular power; which naturally should lead him no farther than their immediate cause. But it seems, the connexion of these changes with certain states of the mind is so constant and necessary, that we pass from the signs of the former directly to the latter.

On this is founded the science of physiognomy, whose first rudiments may be discovered in the unexperienced infant. Nature teaches herself those relations, the knowledge of which is necessary to our existence; and this, among the others, so essential to a social being, who in his original state had no arbitrary language. But the science can be greatly improved by observation; if we may believe what is told us of the people, who have employed themselves particularly in this sort of investigation. The method adopted by one of the most eminent physiognomists in history deserves our attention. It is said, that when he wished to discover any one's inclinations, he used to fashion every gesture of his body into a perfect resemblance with him; and then, from the change produced in his own mind, he collected the situation of the person's whom he imitated. Thus, while by an effort of the will he produced a certain condition of that power, which is the source of muscular motion; he produced at the same time the particular frame of mind connected with it. And may we not, in this way, explain the contagion of passions, so remarkable in many species of sensible creatures? Every one knows how apt we are to catch the gestures of others; and has seen the gaping of a single person spread itself through a whole company. The propagation of convulsive disorders, among people of delicate and mobile habits, is a noted fact of the same kind. And they make it probable, that the motions which accompany the passions are communicated to the beholder; and, becoming certain conditions of the source of muscular action, produce, as in the former instance, the states of mind with which they are connected.

The material principle of motion, in animals, appears to be the same with that of feeling. The voluntary contraction of any muscle in the body is rendered impossible, by dividing the nerves with which it communicates; and every sensation, we know, may be prevented, either by hindering the impression upon our nerves, or by stopping the communication of its effect through them to the brain. All our sensations are, without doubt, so many different states of the mind; but they are the necessary consequences of certain determinate changes in the power we have been considering.

And the faculty of retaining ideas, of comparing, and variously combining them, seems to bear the same relation to the conditions of the common origin of the nerves. It has been weakened, altered, or entirely destroyed, by topical and evident affections of this organ. But, when the memory, the imagination, and the judgment, undergo such remarkable changes from diseases, and from material causes operating upon the body; must we not believe that these qualities of mind, like the others we have mentioned, depend upon certain states of the nervous power, and vary with them? It seems probable, therefore, that every modification of the soul, as far as it relates to the corporeal world, or can become the object of a physician's attention, will be found in some condition of that energy which belongs to the nervous fibre of animals.

They pretend, indeed, that there is no possibility of conceiving either the beginning, or the continuance of motion in our machine, except from the action of an immaterial principle. Yet it is certain that the animal motions would never have begun, and would cease immediately, unless they were supported by the influence of external physical causes. In the impregnated egg is an activity ever discovered, without the application of heat? The soul had slept for ever, and the machine continued motionless, but for the action of this energy. We believe, with the greatest reason, that the brain is the seat of the intelligent principle. But in those fœtuses which are nourished, and arrive at their full growth, without any brain, what shall we suppose to have been the source of motion and activity?

We certainly discover marks of intelligence in every work of Nature; and of these the animal machine appears to be the most perfect and exquisite. Their supreme Author has directed, by fixed laws, to the wisest purposes, the operation of brute and insensible matter. To neglect the investigation of his laws, and refer these purposes to some inferior intelligence, resembles the superstition of those idolaters, who adore in the sun the source of all the beneficent changes in Nature. Such notions are the offspring of ignorance; which leads us to derive from a Being of a superior kind, what we cannot explain from the view of any material energy. And they tend to make that ignorance perpetual, by setting bounds to the progress of the mind in its inquiry after physical causes.

It is in the contemplation of Nature, struggling with the powers which have a tendency to destroy life, that the Animists chiefly triumph. But the efforts of the economy to re-establish its order when disturbed, seem to depend rather on the constitution of the machine, than on any endeavours of the intelligent principle. For they do not vary with the condition of the intelligent principle in different animals, but with the state of the machine. And we certainly observe, that the *vires medicatrices naturæ* are much more perfect in a *polypus* (whose share of intelligence we may believe is not very considerable) than they are in a man. Since, while they cure with some difficulty a small wound in the latter, they form, from each of the divided pieces of the other, a complete animal.

It was easily perceived that every analogy from the sensible world must be insufficient to explain the phenomena of life: and so far there could be no mistake, for the thing rested on simple observation. But they drew too hastily their conclusion, that the phenomena must certainly depend on an immaterial cause. For as yet physicians had given very little attention to that peculiar energy, which resides in the nerves of animals; and though a material agent does not, as far as our observation goes, resemble any other in nature. Was it then surprising, that in their attempts to explain operations, which, however subjected to the common laws of matter, must still be influenced by this vital power, they were so often contradicted by experience? They resembled the speculative mechanic, who, having constructed a machine with a view only to certain principles, is disappointed on trial by the influence of some power which had never entered into his calculation.

One is ready to imagine, from what we know of digestion, that this process can be explained by the facts which relate to mixture, and to fermentation. Yet an excellent anatomist,* who has inquired, by experiment, into the state of this function in living animals, declares that it is not analogous to any chemical operation; and, to mark its peculiar nature, he has given it the name of Animalization. But digestion seems to be, in this sense, a chemical, and a fer-

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* Mr. J. Hunter.

mentative process; as it changes the qualities of certain an
mal and vegetable substances, in a determinate way, reducin
them constantly to a matter of the same kind. We cannot e
plain its distinct and specific nature, because we are not a
quainted with all the powers which concur in the operatio
We calculate pretty well the effect of the animal heat, the ai
the motion; and, since the ingenious experiments of S
John Pringle, of the saliva mixed with our food. The i
fluence of other fluids, which may be present in the stomac
is more obscure; and we know still less of the effect of th
vital power. We perceive, indeed, that nothing favours d
gestion more than a cheerful serenity of mind; while a
afflicting piece of news at once disturbs it, and seems t
change the state of fermentation in the contents of the st
mach: and we know that passions of the mind can onl
affect this function through the nervous influence. Th
abovementioned ingenious observer discovered, that th
process of animalization was more perfect, and sooner com
pleted in a vigorous animal than in one that was weaken
ed; in a hungry animal, than in one which showed no a
petite. But vigour and hunger can only be referred to ce
tain states of that power, which is the source of motio
and of sensation. It has therefore naturally, we may su
pose, a considerable share in this operation. And, as w
cannot pretend to imitate its action out of the body, w
must not expect to find a perfect analogy with digestion i
any artificial process.

But if those animal operations, which discover the great
est affinity with what is observed in the other parts of Na
ture, are still very different, and seem to owe their distinct
ness to the nervous influence; is it not surprising, that th
consideration of this power should have been so long ne
glected by systematics? The simplest observation must hav
informed them, that sensation, and spontaneous motio
distinguish animals from every other kind of being. Was i
not, therefore, reasonable to believe, that in explaining th
animal operations, the facts which relate to sensation an
motion must be of principal importance? But they were le

occupied with examining and comparing the animal phenomena, than with attempts to illustrate them by known and obvious facts. Mechanics and chemistry, supplied a large field for analogies. Some of their principles were ascertained; and to men possessed with the idea of the efficacy of these powers, and of these alone, their application in the theory of the animal functions appeared to be easy, and natural. For the same reason, that the first theory of chemistry was taken from the mechanical properties of bodies, the first theories in physic sprung from our knowledge of the other parts of nature; and were either mechanical or chemical. But the laws of the nervous power could only be determined by attending closely to the phenomena of life, and to that relation between them which might allow their being associated under general heads. The labours of the illustrious Haller were the basis of this work; which has since been enriched by other observers. And in the hands of a professor eminent for his genius, and his extensive knowledge of the animal economy, it has grown into a system: whose principles are as true as any in chemistry or mechanics, being founded, like them, on the sure ground of observation; and are of far more extensive application in the theory of physic.

We have seen the connexion of medicine with the other sciences, and how naturally its improvement follows their progress. We acknowledge ourselves indebted to philosophy, for many rational views of the economy, as well as for all our knowledge of external agents, the means of life, and fruitful source of diseases. Theory was nursed by it, and grew up under its wings. Mankind seem to have an instinctive notion of the relation subsisting between every part of nature; so that the view of one of her operations always suggests some opinion concerning another. This movement of the soul will often precipitate us into error; but it is constant and necessary.

What can we reason, but from what we know ?

When it was believed, that fire and water were Nature's

tion. The phenomena which occurred to the most su
cial view; as the evacuation of different fluids in diff
states of the constitution; and the various appearan
the countenance, that seemed to indicate a variety i
proportion of the animal fluids; supplied this basis.
thus the terms, sanguine, phlegmatic, choleric, and me
cholic, which carried a more particular reference to the
ditions of the economy, were associated with the for
The terms of the ancients have descended to us, so sa
fied by long usage that we continue to employ them; tho
in general, they convey certain collections of ideas
different from those to which they were originally anne

When chemistry began to make some progress, and
not yet acquired just principles of its own, its adepts
lieved, through ignorance and enthusiasm, that they c
discover in it the principles of every other science. An
these rude philosophers, from the relation of chemistr
the materia medica, were particularly connected with
dicine; they presently introduced into its theories the m
striking phenomena of their art. Every appearance in
economy was traced to some ebullition, or effervescen
some effect of an acid, an alkali, nitre, or sulphur.

Mechanical notions are so extremely obvious, their
jects so familiar to the senses, and concerned in our us
occupations, that we naturally recur to them first for
explanation of any phenomenon. They were soon

Our attention to electricity particularly, and its considerable improvement in the present age, have they not contributed towards turning our thoughts to the nervous system? For though it is by no means demonstrated, that the nervous power is the same with that which occasions the phenomena of electricity; and the laws of this power are as independent on any such hypothesis, as the laws of gravitation are upon the hypothesis of an etherial fluid; yet the contemplation of such active and subtile energies has enlarged our views, and drawn the attention from those principles of mechanics and chemistry, to which it was before too slavishly attached.

Thus medical systems put on different appearances, according to the different lights which they receive from philosophy. But these reflected rays, too weak of themselves to dispel the darkness, could only produce indistinct, and frequently monstrous images. It was not till the sun arose in our own hemisphere, and that spirit of observation, which has distinguished the latter ages of Europe, began to exercise itself on the animal machine, that we acquired clear and accurate notions. The ignorant simplicity of the ancients, the mad whimsies of the chemists, and the more solemn fooleries of the mathematicians, disappeared by degrees; and were forced to quit, though reluctantly, their hold on the minds of men. We learned to fix the bounds of those analogies, which we drew from the other parts of nature; nor was our progress stopped when the light of analogy failed, and there seemed to be an end of all connexion of the animal system with the rest of the world. Observation still conducted us, in our attempts to collect the laws of the economy from a full induction of facts.

This sort of knowledge is so valuable, that every endeavour to extend it is meritorious. It will show a laudable inclination, and some degree of judgment in the choice of our road, though the state of our abilities, or opportunities, may deny us the praise of a considerable progress. And even when we fail of producing a clear conviction, it is rare that, in the collision of facts, no sparks are struck out to show a future adventurer his way.

II. *Observations on Retroversion of the Uterus, &c. as ad before the Academy of Medicine, 18th Dec.* 1820. By WILLIAM P. DEWEES, M. D.

THE disease on which we are about to offer a few observations, has only been accurately known, since the year 754. At this time the late Dr. Hunter met with a case of death from this cause, which so powerfully excited his interest, that it led to a complete and accurate knowledge of the changes induced upon the uterus, which constitute it. Since this period, there is scarcely a writer on midwifery that does not notice it; yet notwithstanding this familiar mention of the complaint, practitioners are by no means agreed, either as to its importance, or its particular management. Indeed, there is scarcely a disease to which the human body is liable, on which there is such a discordancy of opinion. While one set of practitioners* view it as an accident of the most serious kind, another regard it as a matter of little concern or importance†—both cannot be right; but on which opinion shall the young practitioner repose confidence? He will be distracted by discrepancy, and will either negligently wait, and trust to the powers of nature, or will unnecessarily subject his patient, to both pain and hazard.

With a hope that this variance of opinion may without difficulty be reconciled, the present observations are ventured.

In prosecuting our purpose, we shall, first, give the history of this complaint. Secondly, animadvert upon Dr. Denman's theory and mode of treating it. Thirdly, offer some observations on the mode of reduction, &c.

It is now no longer subject to question, that this complaint can take place, with the unimpregnated uterus, though the impregnated is much the most liable to it, as it necessarily offers a larger surface to be acted upon by the causes which may be capable of producing it.‡

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* Hunter, Baudelocque, Meygrier, Burns, &c.

† Denman and Merriman.

‡ Baudelocque, Denman, Merriman, Burns, &c.

The remote causes are all those, which may tend to depress the fundus of the uterus; and may be external violence, such as blows, pressure, sudden exertion, &c. or they may be violent efforts to vomit, or incessant coughing; an over distended bladder, or perhaps an unusual accumulation of fæces in the rectum or sigmoid flection of the colon.

When either of these causes act so as to produce the retroversion, it is by carrying the fundus backwards and downwards, so as to place it between the vagina and rectum, by making it fold upon itself—it will follow then as a consequence, that if the fundus be thus depressed, the neck or mouth of the uterus will be elevated, in an opposite direction, and will be found on the anterior and superior part of the pelvis, and immediately almost behind the symphysis pubis. The fundus may have different degrees of depression, and the mouth of elevation—hence some have divided this complaint into complete and incomplete retroversion.* This change in the situation of the uterus may be induced suddenly, or it may occupy considerable time before it becomes established. In three instances we saw it almost instantly produced; in one by a severe blow across the back while stooping; in the second by attempting to lift a heavy tub of clothes; in the third by an effort to jump a fence, when pursued by a Bull.† Baudelocque and others furnish us with instances of its gradual but evident approach to retroversion.

The symptoms which accompany this displacement of the uterus are more or less intense, as this viscus may be larger or smaller, or as it may be of recent occurrence, or of long standing. When suddenly induced, the symptoms are, for the most part, violent and alarming. In the cases we have seen, there was a stoppage of the urine and of the fæces; pains resembling those of labour; and a disposition to syncope; nor was there much alteration in these symptoms until the fundus of the uterus was restored. When it is a long time retroverting the symptoms are milder, and only acquire intensity when the displacement has become complete. During the progress, and sometimes even after the change has taken place, the sufferings of the woman are not extreme. We find her complaining of a difficulty in making water, with an increase of desire to do so; a painful dragging pain about the hips, loins, and thighs, and a forcing bearing down pain resembling labour. These symptoms may continue for a considerable time without augmented suffering; but this mild condition of the complaint must necessarily have a limit, should the fundus not spontaneously restore itself; for if the uterus contain an ovum, it will go on to be developed for some time, with nearly as much certainty and rapidity as if this accident had not happened; but it will follow as a consequence, that the inconveniences just mentioned will increase in proportion to the augmentation of the uterus, so that there will an entire stop be put to the evacuation of both urine and fæces; this will provoke an intense desire to discharge both the one and the other, which so far from being effective, will but increase the difficulty, by forcing the uterus lower and lower, and thus compressing the neck of the bladder and rectum with still more strictness; this will be accompanied with extreme pain, and if the patient be not speedily relieved, the bladder will either burst,* or suffer from inflammation and gangrene,† and death quickly ensue.

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* Hunter's Med. Obs. vol. ii. Meygrier, Nouveaux Elémens, p. 116, vol. i.

† Baudelocque, Meygrier, Dr. Evans's Med. Com. vol. vi. p. 215, &c. &c.

In the unimpregnated state of the uterus, the symptoms, as far as my experience goes, never arrive at this melancholy height; and for this plain reason, that the uterus in such cases never, I believe, acquires so much size as to entirely and intimately occupy the lower portion of the pelvis, and consequently cannot so entirely obliterate the urinal or fæcal canals. But, although the symptoms which attend this particular condition of the uterus be less severe, it would seem that the parts never become reconciled to this change of situation, however long they may be subjected to it; at least this is the case with a patient now under my care. This lady has at this time all the milder symptoms which accompany this disease, together with considerable leucorrhœa. The uterus has been retroverted most probably ever since her last labour, a period of nearly eight years. From her history of herself, it would seem to have been produced very soon after delivery, for then all the inconveniences which she now suffers began to manifest themselves; nor from that time to the present moment has she experienced any alleviation. Her case I fear to be irremediable, as I apprehend adhesions are formed which will prevent the restoration of the fundus, at least it has hitherto resisted moderate efforts to reduce it.

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* Mrs. Lynn's case. Med. Obs. and Inq. vol. v. p. 388. Doeveren as quoted by Merriman.

† Dr. Bell's case. Med. Facts, vol. viii. p. 32.

The most usual period of occurrence in the impregnated uterus is from the second to the fourth month of gestation; for after this period, its volume will be such, as to preclude (cæteris paribus) the possibility of its happening. At this time the fundus of the uterus is about to emerge from the inferior strait, and may be felt immediately above the pubes, and has now a kind of resting place offered it by the projection of the sacrum. But that it has happened after this period cannot be questioned; Smellie* furnishes us with an instance at the fifth month; and I myself saw a case in consultation with Dr. Gallaher at between the sixth and seventh months. But in these deviations, we must seek for the cause of the retroversion, either in the unusual size of the pelvis, or the small size of the ovum, from retarded or deficient development. In the patient just mentioned there was retarded development, from her labouring under a confirmed phthisis pulmonalis; her cough was severe; and in a fit of more than ordinary severity and length, the uterus was retroverted; the symptoms from it were violent, but they ceased immediately upon the restoration of the fundus; the patient died about two weeks after. This furnishes an example of retroversion without previous suppression of urine.

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* Vol. ii. p. 133.

be "obliquely upwards and backwards;" now had it be
a genuine retroversion, the direction would have be
backwards and immediately downwards. And this meth
may be practised in cases of extra uterine pregnancy, wi
perhaps even more certainty of success; for in a case
this kind related by Dr. Kelly,* it was found on dissecti
that instead of the ovum carrying the uterus downwar
it was found raised up in the pelvis by the head of the chil
and "had the usual appearance of a womb in the unimpre
nated state." Now, in a case like this, there could have be
no difficulty in passing a catheter into the uterus, which
once, from its direction, would decide it was not retrovert

From what I have seen of this disease, and from atte
tively considering a number of the cases upon record,
would propose, that this method should be adopted, to
termine the nature of the tumour found behind the vagi
and which leads to the ambiguity just mentioned. It is
questionably highly important to determine the point
question; and more especially, early after the untowa
symptoms manifest themselves—for if this be neglected
retroversion, it may put it out of our power to profit by t
knowledge; and if it be either an ovarial tumour or an
tra uterine ovum, it may lead to the employment of mea
that may relieve the suffering patient.

A confidence is felt, that the method just proposed wo
dissipate all doubt upon the subject, and lead us to t
kind of certainty as to the nature of the disease, as wo
at once decide the mode that should be resorted to
relief. As far as I have been able to determine by
amining the histories of ovarial and extra uterine tumou
there has not occurred an instance of the fundus of
uterus being depressed by them; consequently, determini

it was eventually extracted; the mother however died; upon dissection, the uterus was found " driven upwards and forwards by the sacculus" which contained the fœtus; and it was observed " that there was a fullness and hardness very perceptible to be felt outwardly in the fore part of the belly, some distance below the navel," and this was found to be the uterus. We may then, perhaps, in these cases be always able to feel the uterus immediately over the pubes, and this may serve an important purpose as regards diagnosis—but this is purely conjectural—yet it may deserve attention.

We may also observe, that in cases of extra-uterine ova, or ovarial tumours, their progress is slow and pretty regular—the latter especially is almost always very slow, consequently the symptoms which might confound them with retroversion, must be very gradual in their approach, and must occupy a long time before they create serious difficulty—the history of the case then, may furnish us with important facts.

Having thus briefly given the history of the retroversion of the uterus, we shall now offer a few observations on Dr. Denman's theory and mode of treating it. He says, " There is in every case a suppression of urine, with extreme pain, and by its continuance such distention of the bladder, that the tumour formed by it in the *abdomen* often equals in size, and resembles in shape, the *uterus* in the sixth or seventh month of pregnancy. But it is necessary to observe, that the suppression of urine is frequently absolute only before the retroversion of the *uterus*, or during the time it is retroverting; for, when the retroversion is completed, there is often a discharge of some urine, so as to prevent an increase of the distention of the bladder, though not in sufficient quantity to remove it."*

In this account of the retroversion, it will, I trust, be made to appear, that Dr. D. has forced fact to comport with his theory of the production of this disease; I shall therefore first state his opinion upon this subject, that our ob-

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* Introduction, p. 137.

effected by a distention of the bladder from an accumu
of urine.* Now it is essential to this theory, that a s
pression of urine should exist before the retroversion t
place, and that the stoppage should only be "absol
then "or during the time it is retroverting." We w
inquire what gave rise to the suppression of the urine, w
is so essential to the retroversion? Not a word is sai
explanation of this question. Does it really exist at
time Dr. D. supposes, so as always to produce the dis
in question? We think we have, from our own experie
as well as that of others, strong reason to doubt it.†—F
because no one, with the exception of Dr. Merriman,
corroborated his opinion by adopting his explanation.—
condly, Baudelocque declares, that in a certain marchio
the retroversion took place instantly, and "from that
ment she found it impossible to evacuate a *single dro*
water." And this is conformable to my own experienc
at least four cases.—Thirdly, Baudelocque tells us he
monstrated a slow inversion‡ of the uterus to his pu
and that the inversion was not complete till after thre
four weeks. He does not mention any difficulty in mak
water, nor a suppression of it, which, agreeably to Dr.
must be absolute "before the retroversion, or during
time it is retroverting." Now, had either of these circ
stances obtained, it would, from the known accurac
Baudelocque, have been mentioned; but, on the contr

plete "that the woman found herself obliged to submit to the necessity of reducing it."

Dr. D. declares "the uterus must be elevated before it can be retroverted."* To disprove this, it is only necessary to recur to the cases we have already mentioned, where it was instantaneously induced; and where it is impossible to conceive that the uterus could be elevated. Notwithstanding this, we would agree with him, that it could be more easily retroverted when this condition exists. We mean then merely to insist, that elevation is not a *sine qua non* to retroversion, and thus disprove a part of Dr. Denman's theory.

But if we admit Dr. D.'s diagnosis of this disease, we shall perhaps be forced also to admit the justness of his general positions; we shall therefore immediately advert to them. He says, "If a woman, about the third month of pregnancy, has a suppression of urine, continuing a *certain* length of time, and producing a *certain* degree of distention of the bladder, we may be assured that the uterus is retroverted."† This is a notable instance of the want of precision; for we cannot possibly determine the cause from the consequence; for the employment of these conveniently ambiguous words, "*a certain time*" and "*a certain degree*," leaves to us the choice of either. If a suppression is so complete as to produce "a certain degree of distention," then we may be assured "that the uterus is retroverted," provided the woman is three months advanced in her pregnancy. And why are we assured that this is the case? Is it because a suppression of urine will always produce a retroversion, or because a retroversion will always occasion a suppression of urine? Now the first, we know, is not strictly the case, as we have many times known a suppression of urine without a retroversion; but we have never known the converse of this. Or shall we suppose Dr. D. to mean, that if a "certain degree" of distention of the bladder be continued a "certain time," that is, long enough to produce a retroversion, "we may be assured" there is a retroverthe disease, he may well be justified in leaving it to nat

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* Introduction, p. 139.

† Ibid. p. 139.

Dr. D.* says a little further, "But the preceding
pression of urine may be overlooked, as there is not o
sion for it to be of long continuance in order to produc
effects, especially in a woman who hath a capacious pe
in whom the retroversion of the uterus is most likel
happen." Can we imagine that so distressing a circ
stance as a suppression of urine could possibly be overl
ed, though there should "not be occasion for it to b
long continuance in order to produce its effect?" Let
one who has experienced this inconvenience for even a
hours, be asked whether he remembers ever to have
suffered, and his answer will speedily convince us it is
easy to be "overlooked." We cannot then believe it wo
be so with a woman, merely because she has a "capaci
pelvis:" for a pelvis of any capacity cannot relieve the
arising from a suppression of urine, though this be "
necessarily of long continuance." If the pelvis be
large, it may give rise to a retroversion at a more advan
period of gestation than a smaller one; and this is all
it can do: for if we understand the import of a "capaci
pelvis," it means one rather beyond the ordinary stand
and if this be true, we deny any greater facility to re
version, in such a one, than in one of the ordinary m
surement at the period fixed for this accident by Dr.
himself: for at the third month there is abundant

Dr. D.* observes, "It has been said that the state of retroversion was injurious to the uterus itself, and would produce some dangerous disease in the part: it has also been asserted that, if the uterus was permitted to remain in that state, it would be locked in the pelvis by the gradual enlargement of the ovum, in such a manner as to render the reposition impracticable, and the death of the patient inevitable." He contends that,† "for both these consequences there cannot surely be reason to fear; for if the uterus be injured there will be no further growth of the ovum; and if the ovum should continue to grow, it is the most infallible proof that the uterus has not received any injury."

From this statement a young practitioner would irresistibly be drawn to the conclusion, that no evil would arise even from an unreduced uterus, than which nothing can be further from the truth. We shall, we trust, prove that neither member of this dilemma is founded on fact or upon correct observation. We will, therefore, examine them a little more at large.

Dr. Denman tells us, that, "If the uterus be injured, there will be no further growth of the ovum"—from this it would be natural to conclude, that if the growth of the ovum were interrupted, all the evils that could arise under the circumstance of retroversion would be at an end, and that consequently it would be desirable that this should happen, since agreeably to this it would effectually put a stop to the mischief that would ensue if it were progressive in its development. It is true that the meaning we have given to this passage is by implication, but we sincerely think the premises warrant it—for what are we to conclude from it, if it bear not this construction? If the uterus be injured by the retroversion, it would seem to be premised, that this injury would be repaired by the ovum ceasing to increase. Now to us it appears that two positive evils would arise from this condition of the uterus—the one, the mischief that is done to the uterus itself; and this mischief we cannot limit at pleasure, for if it be so great as to cause the death of the ovum, it may also be so great as to cause the death of the patient. The second is, the destruction of the ovum; which when it happens, must sooner or later eventuate in abortion; and the consequences of abortion while the uterus is retroverted, are sometimes very serious indeed.

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* Introduction, p. 143.

† Ibid. p. 143 and 4.

If risk is to be incurred from the uterus throwing off its contents, let it be from pursuing a plan that offers at least some probable chance of relief to the patient—namely, the attempt at reduction—for should we succeed, abortion may not follow, for (as we shall remark more at large presently) it is not a necessary consequence; but should it take place after the restoration, the sufferings of the woman will be much diminished by the natural position of the uterus being restored: and should we fail in our attempt at reposition, the patient will be in no worse situation than before.

In confirmation of this, we shall relate an interesting case given by Dr. Bell.* We shall take the liberty however of abridging it, but shall not suppress any material point.—A woman in the 36th year of her age and mother of nine children, was attacked with great pain and difficulty in making water, for which Dr. Bell prescribed several remedies without seeing of her. She however became worse, and he was requested to visit her, which he did accordingly. He found her in much distress and pain about the abdomen, &c. with frequent vomiting and obstinate constipation. Upon examination per vaginam it was found that she was labouring under a retroverted uterus.—Every thing was done that the necessity of the case seemed to suggest, but without the smallest benefit, as the attempts at reduction were unavailing.—Symptoms of labour soon supervened, and after a short continuance, the fœtus was expelled, and the patient died a few hours after. Leave was obtained to inspect the body, and the following is the account of the dissection in Dr. Bell's own words. "On turning up the intestines, the bladder was discovered flaccid, and much enlarged. Here the inflammation had made great progress. The peritonæum lining [covering] the bladder, and for a considerable distance round it, was covered with a strong, thick, inflammatory crust. The coats of the bladder were every where much thickened, and upon the posterior and superior part of it, there was a portion of the size of a half crown, in a state of mortification."—" There was very little urine in the bladder. The uterus was found to have regained its natural situation in the pelvis. This organ appeared not to have suffered from inflammation." Now the death of this patient could only be attributed to the injury arising from retroversion.

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* Med. Facts, vol. viii. p. 32.

Let us now examine the second member of Dr. D's dilemma.—" Should the ovum continue to grow, it is the most infallible proof that the uterus has not received any material injury." Consequently we are to conclude, that no future mischief can arise. But does this rest upon that foundation on which it should alone rest, experience?—Certainly it does not. The uterine ovum goes on to be developed as we have already noticed, and eventually, if not interrupted, will fill up the whole of the inferior strait—its volume becomes too great to pass through the superior strait; it will completely obliterate the rectum or allow none but the very thinnest fæces to pass; the urethra, or neck of the bladder, will be so compressed as to interrupt the flow of urine; the consequence is, that the bladder will suffer, as has already been noticed, and most probably *death* will be the result.

It is a matter of no consequence in the discussion of this subject, which organ of the pelvis suffers so as to produce death—it is the uterus being in a state of retroversion, that creates the mischief, and this is all that is necessary to contend for.

But has Dr. D. or any body else, seen an instance, in which the uterus continued to develop itself, which was not ultimately attended with serious distress, or perhaps death? *we* know of no such instance upon record.

When a gentleman sets out determined to attach his own detriment than what may be occasioned by the tempora interruption of the discharges by stool and urine."[*] No we would inquire what inconveniences can well be cons dered greater, than a suppression of urine, or an incapacit to pass fæces? Dr. D.'s attempt to modify this unpleasan and often hazardous condition, by employing the wor "temporary," will not alter the nature of the sufferings the patient, nor diminish them, either in point of duratio or intensity—and she will not consider the endurance pain "for many days or weeks" as "temporary." But may be said, that the word "temporary" will not conve the idea of such extended suffering; perhaps at first sig it will not, nor was it intended it should; but let us recu to the fact about which there can be no dispute, that th inconveniences stated are always as permanent as the influence of the cause which produces them; and consequently, that if this continue, "the interruption to the discharge by stool and urine" will continue; therefore, "temporary in this instance must mean a period commensurate wit the duration of the cause.

Dr. D.[†] again observes, "that the enlargement of th uterus, from the increase of the ovum, is so far from obstructing the ascent of the fundus, that it contributes t promote the effect, the distention of the cervix becoming balance to counteract the depression of the fundus; for have found no cases of the retroverted uterus admit of reposition with such difficulty as in women who were no pregnant, in whom the uterus underwent no change." Baudelocque tells us, "though we meet with few obstacles t this reduction when the displacement is recent, and th volume of the uterus still small, they are very great, and sometimes insurmountable, when it has existed several days or weeks." The experience of Baudelocque was at least equal to that of Dr. D. Here we are led to conclude, that so far from the progressive development of the uterus being an injury, it must be regarded as a positive benefit; and were Dr. D.'s statement strictly correct, it would be so—that is, if the "cervix" should augment so much that its weight would exceed that of the fundus, and thus from its gravity restore it. But this is far from being the case, for there cannot be the smallest advantage derived from the "distention of the cervix" as an antagonizing power to the fundus; for the latter will acquire weight in a precise ratio with the cervix, and they will consequently be equal to each other. Indeed we may go farther, and consider it a positive evil; for both reason and experience support us in the opinion, that the accidents attendant upon retroversion are augmented in violence by the increased size of the ovum. In proof of these we need only refer to the situation of the "cervix" as regards the bladder; and to the history of those cases, in which this latter organ has materially suffered. With respect to the first, it will easily be understood how an increased size of the "cervix" shall still more incommode the bladder, by pressing upon its neck with augmented force; and as regards the second, it will be found that the bladder has never materially suffered but where there was an increased bulk of the uterus. Now if this be true, and we sincerely believe it to be so, where is the advantage proposed by "the enlargement of the uterus?" Nor does Dr. D.'s opinion derive the smallest support from the instances which he adduces for this purpose; namely, that empty uteri are more troublesome to reduce than impregnated ones; for after carrying up the fundus of an impregnated uterus above the projection of the sacrum, its bulk is an advantage, for by its occupying a larger space in the superior strait, it is less liable to relapse into its old situation; besides, its elasticity aids its restoration, for being bent down upon itself, something like a retort, it is constantly disposed to restore itself whereve freed from restraint; unless indeed it has so long submitte to this situation that its residiency is entirely destroyed The unimpregnated uterus has not this advantage, for it i not so much bent, nor does it occupy so much space; bein then more at liberty, it has a stronger tendency to fall bac as from its size, it cannot rise above the projection of th sacrum; it is not, then, as far as my experience goes, mor difficult to restore the fundus, but it is confessedly mor difficult to retain it in its situation, after reduction.

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[*] Introduction, p. 145

[†] Ibid. p. 145.

The following declaration of Dr. D. does not coincid with my own experience, nor is it corroborated by the observations of others. "If the attempt to replace the uteru be instantly made after the urine is discharged, so muc force will often be required for the purpose as will, notwithstanding all precaution, give much pain, induce the hazard of injuring the uterus, and often occasion abortion. If this sentence be literally taken, it would seem to inculcate that, the moment after the discharge of urine is an improper instant to attempt the reduction; yet it is directe by all writers and practitioners to draw of the water, whenever practicable, before we make an effort at reduction; an Dr. D. himself must be understood to give the same advic when he says "that all attempts to restore the uterus to it natural position, before the distention of the bladder is removed, must be fruitless, as the uterus will be irresistibl borne down by the pressure of the superincumbent bladder The first step then to be taken for the relief of the patien is to draw off the water."[†] Yet he tells us, as just stated that "if the attempt to replace the uterus be instantly mad after the urine is discharged," we run the risk of provokin abortion. How would a young practitioner act under thi contradictory advice?

But we will not insist further on this inconsistency. Le us now inquire whether the apprehensions of Dr. D. wit respect to abortion often following the attempt at reduction be well founded. I have never seen it follow in th

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[*] Introduction, p. 146.

[†] Ibid. p. 141.

cases which have fallen under my notice in any one instance. Baudelocque[*] tells us, "that abortion is not always the consequence of such efforts," and declares he could cite more than twenty instances in which it did not follow. Dr. Hunter tells us, "I have known several cases of the same kind," (that is of retroversion,) "but in a less advanced state," (than the one attended by Mr. Wall and himself.) "They all happened about the third month, sooner or later." "They were all *successfully* treated after the following manner;" and then proceeds to describe his mode of restoring the fundus. But suppose abortion should take place, it does not necessarily follow that our exertions have provoked it; for in the case mentioned by Smellie, this happened after the uterus had spontaneously replaced itself; and it is considered by almost every other practitioner than Dr. D. to be an imperious indication, to restore the fundus of the uterus, nor do they regard the *contingency* of abortion but of minor consequence. Meygrier[†] tells us "Il ne faut pas craindre de provoquer l'avortement par des tentatives réitérées; elle sont bien moins dangereuses pour la mère et l'énfant que la non reduction de la matrice." Dr. Hunter was of the same opinion.[‡] But Dr. D.[§] confesses that the fundus may be restored in some cases "without much force," but that in others every attempt has failed. To what is this failure in general to be attributed? We believe to the advice of Dr. D. himself, to treat the complaint as one of little consequence, and to temporise until the uterus becomes so enlarged, and so inveterately fixed, as to render every attempt at reduction unavailing. I think we may with much safety conclude, that the alternative offered for reduction by Dr. D. is much more inconvenient, if not hazardous to the patient, than well directed efforts to restore the fundus. The repeated and daily use of the catheter is not only very troublesome, agreeably to Dr. D.'s own confession,[||] but in many instances so highly repugnant to deli-

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[*] System of Midwifery, vol. i. p. 176.

[†] Noveaux Elémens, vol. p. 122.

[‡] Medical Observations, vol. iv. p. 406.

[§] Introduction, vol. i. p. 146.

[||] Introduction, vol. i. p. 147.

the evils which could attend an early attempt now sup
vene with aggravated force; and the patient, after long a
terrible conflicts, at last succumbs.

It cannot be too strongly insisted upon that the pati
should be carefully watched, lest the period may pass
which manual assistance may be successful. We may, wi
out running much risk, temporise for a time; but let
that time be exceeded. We are of opinion it should go
ry little beyond the fourth month: for after this period,
bulk of the uterus may be found greater than the openi
of the superior strait. But should alarming symptoms
pervene at any time previously, they must most promp
be attended to. Were this rule strictly adhered to, we
lieve we should seldom hear of a fatal issue to this disea
When death has occurred, it has almost always been fr
neglecting the proper period for reposition. This was t
case with Dr. Hunter and Mr. Wall's patient.[*] It was
in two other instances mentioned by Dr. Hunter;[†] also
the case related by Dr. Bell;[‡] so also in Dr. Orr's[§] patie

But, concludes Dr. D.[‖] "we may bring the matter
this issue: if the uterus, when retroverted, can be replac
by art, without the exertion of much force, or the risk
mischief, the immediate reposition, *though not absolut
necessary, is at all times an event to be wished;* as fart
apprehension and trouble are prevented, *the safety of
patient ensured*, and her mind quieted. But (continues

niences produced by the retroversion." The paragraph we have just quoted is followed by this remarkable observation, "We shall also find, that the longer the attempt to replace the uterus be delayed, the more easy the operation will ultimately be, and the success more certain." In one place we are taught to believe that "immediate reposition" is desirable, as the safety of the patient is thereby "ensured;" in another, we are advised to withhold this prompt assistance, "for the longer it is delayed the easier and more certain will be the operation." We must now ask, why is the immediate reposition at "all times to be wished," if the longer we delay should render the operation both easier and more certain? What would be the impression of an inexperienced practitioner who may have resorted to Dr. D. for information? How could he decide on which of these contradictory opinions he should rely?

From what has been said, I think we may fairly conclude from D.'s own statements, that the woman labouring under retroversion does run a risk of a serious kind by the long continuance of the disease; and that risk so great as to threaten, and sometimes to end in the loss of life. We agree with Dr. D. that this danger does not arise always, nay perhaps not very often, from the injury which the uterus itself sustains, but from the injury inflicted upon the bladder from long continued pressure, and distention. But it is a matter of very little moment which of these organs suffer, provided one or the other will suffer, from the derangement we are speaking of. The bladder, from its peculiar structure, can suffer but a certain degree of distention, without experiencing serious mischief; and if the distraction of its sides be forced beyond this, it may become inflamed,[*] gangrenous,[†] or burst[‡]—now, as the uterus when retroverted always presses the bladder with more or less force, it will necessarily suffer in proportion to that compression[§]—if this be to the extent of entirely obliterating the urethra, so that a catheter cannot pass, the conseque just mentioned must ensue. We do consider then, so lo as the impregnated uterus be in a state of retroversion, th is no safety for the bladder, and consequently none for woman. We shall here close the second part of our quiry.

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[*] Dr. Bell, Med. Facts, vol. iii. p. 32.

[†] Mr. Lynn, Med. Obs. and Inq. vol. v. p. 388.

[‡] Dr. Squire, Med. Review for 1801.

[§] Deeveren's case, as quoted by Merriman, p. 13. Essay on Retroversion.

We shall now proceed to make a few observations up the mode of reduction, and attempt to reconcile the disc dant opinions that are entertained in regard to retroversi of the uterus.

From a careful investigation of the cases upon reco and from our own personal experience, we are convin that Dr. D. has done no inconsiderable mischief, by tre ing this complaint as one scarcely meriting considerati It has diverted the public mind in many instances from th attention to the situation of the female while labouri under the disease, to which it is so justly entitled, a thereby permitted the time to pass by, at which assistan would have been available; and death has been but often the melancholy consequence. That the disease is longer so formidable as formerly, we grant; but this is n owing to any change in the nature of the complaint, but its being better understood, and to its being more promp attended to. On the continent of Europe this disease constantly regarded with a suspicious eye, and is rare permitted to produce fatal consequences. They there un versally look upon it as a disease of danger, and treat accordingly; the effect of this is, they have much few victims.

It is a fact as remarkable, as important, that almost the fatal histories of retroversion upon record are to found in the British publications; to what can this be owin but to the difference of pathological views as entertained the continent of Europe, and in the British isles? We not wish to deny, that Dr. D. has great merit in having successfully investigated the mechanism of this accide but at the same time must believe that it led him to rega it, as not only of easy production, but of easy cure. In pro of this we need only repeat his diagnosis, that "if a woman, about the third month of pregnancy, has a suppression of urine continuing for a certain time, and producing a certain degree of distention of the bladder, we may be assured that the uterus is retroverted." If Dr. D. has uniformly determined the presence of retroversion from these symptoms, without an examination per vaginam, we cannot be surprised at the little consequence he attaches to it, or at the many and wonderful cures performed by nature or the mere introduction of the catheter. The symptoms just adverted to, should always put us upon our guard; for the uterus may be retroverted, and it should prompt us to an examination, to put it beyond doubt. But they never justify us to declare this to be the case, without this examination; for I have many times known this difficulty of making water in pregnant women, relieved even without the catheter by the mere exhibition of the sweet spirits of nitre and a little laudanum; and I have in several instances examined per vaginam, when it was more than ordinarily severe, without finding a retroversion. I am, therefore, entirely convinced, from an extensive experience, that a suppression of urine may take place at the period designated by Dr. D. and that "a certain degree of distention of the bladder" may be produced (for I have been obliged occasionally to use the catheter) without inducing retroversion. Many have therefore been misled, by believing the partial symptoms assigned by Dr. D. to constitute the disease; these symptoms are of frequent occurrence in pregnant women, while retroversion is comparatively rare.

But let us agree with Dr. D. as to the cause of the disease; does a knowledge of it, render it either lighter or of less moment? certainly not—nor will the removal of the urine alone, in nine cases out of ten, be sufficient for the removal of the disease, although it becomes indispensable to the prevention of serious mischief, or to the mechanical *restoration* of the fundus; and a reliance upon it has but in too many instances proved fatal or seriously mischievous to the patient—for we must again insist, that the bare removal of a distended bladder is not alone sufficient. No the case ought fairly to stand thus; if the plan propos by Dr. D. does not always succeed, and if, when it do not, the patient by the increase of the ovum is always se ously deranged by it, and her life not unfrequently lo and if upon the plan we shall presently propose, no such i conveniences shall accrue, but on the contrary the mo decided benefits result, ought we for a moment to hesita between the two?

It appears then certain, that by temporising we incur ri almost always, and invariably an increase of difficulty attempting restoration—does it not appear a deduction n to be resisted, that we should always attempt the latt when the former should not pretty speedily succeed? T time we may perhaps safely indulge in, we have alrea pointed out.

What are the objections to the attempt at restoratio 1st. Provoking abortion by giving pain or doing misch to the uterus—2d. By its being unsuccessful, after havi made strong and repeated efforts to this end. With respe to the first, we have already said enough to convince a unprejudiced mind, that it should not constitute an obje tion. And as regards the second, it has only arisen in co sequence of the proper period being permitted to pass, befo the operation was attempted, or this not conducted after proper manner. We have already said enough as respec procrastination, and shall therefore not repeat it; but will spend a little time in the consideration of the prop mode of conducting the operation.

The usual directions for this are known to every bod it is therefore not necessary to transcribe them. We sh only consider what the forces are which oppose the restor tion of the fundus in our attempts to this end, and th point out what we think the best mode of overcoming the First, a distended bladder. Second, an expanded rectu and perhaps a loaded colon at its great flexure. Third, t counteracting efforts of the patient herself. Fourth, the t great bulk of the uterus.

The mode of overcoming the first difficulty will immediately suggest itself; but however obvious this may be, it is by no means of always easy accomplishment. It requires both care and dexterity. Dr. Denman's* observations on this subject are so judicious, that I feel I cannot do better than transcribe them. "The first step to be taken for the relief of the patient is to draw off the water: yet there is always a great difficulty in the introduction of the common catheter, because the urethra is elongated, altered in its direction, and pressed against the ossa pubis by the tumour formed by the retroverted uterus; and many women, when the uterus was retroverted, have lost their lives from want of expertness in introducing the catheter. But the attending inconveniences may be avoided or surmounted by the use of the flexible male catheter slowly conducted through the uretha. I say slowly," continues the doctor, "because whatever catheter is used, the success of the operation, and the ease and safety of the patient, very much depend upon this circumstance; for if we attempt to perform it with haste and dexterity, or strive to overcome the difficulty by force, we shall be foiled in the attempt, or it will be scarcely possible to avoid doing injury to the parts. The catheter should not be carried further into the bladder, when the urine begins to flow, unless it ceases before the distention be removed, which in some cases happens in such a manner as to give us the idea of a bladder divided into two cavities. External pressure upon the abdomen, when the catheter is introduced, will also favour the discharge of the urine."

For the removal of the second difficulty, it must be immediately attempted to throw up injections; but this sometimes is equally as perplexing as the introduction of the catheter; but the point should not be given up without the thing is really impracticable. In this case we must do without it. But from a case having lately occurred to me, in which the rectum was very strongly compressed by the uterus, though not in a state of retroversion, in which I succeeded in having injections thrown up, by employing a large male gum-elastic catheter for the canula.* I am induced to believe, that by a little careful and skilful management it might succeed in cases of retroversion. It should at all events be tried before the thing is abandoned as impracticable. The injection should consist simply of salt and water in the proportion of a table spoonful to a pint of water. We may also, a few hours before the attempt at reduction be made, give the sulphate of magnesia in small doses, which has sometimes had a good effect, provided the stomach be free from nausea or vomiting.

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* Introduction, p. 137.

The third obstacle we have to contend with is the violent efforts that are induced to bear down; thus counteracting all our endeavours at reposition. This is perfectly involuntary, and is decidedly the greatest hindrance we have to contend with in ordinary cases; indeed it renders the most simple form of this complaint, a case of some difficulty, and I have known it the only one to oppose us. In cases where the urine can be drawn off without difficulty, and the contents of the rectum discharged without much trouble, we may meet with such opposition from these involuntary efforts as either to foil us, or to render the reposition extremely difficult. Indeed, as far as my experience justifies the remark, it is almost the only one I have ever met with. Nor do I stand alone in this remark; it must have happened with every practitioner, though not distinctly stated, as far as my recollection serves me, by any but Dr. Bell, who tells us, "the pressure (employed in the reduction) however, excited strong efforts of bearing down, which were a considerable bar to the success of the operation; and at one time, when the uterus appeared to be giving way, and I had great hopes of success, the patient, by an involuntary forcible effort of this kind, destroyed all the advantages I had gained."

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* I am originally indebted to my much respected friend, Dr. Physick, for this improvement in the administration of injections.

From the consideration of this opposition, rendering every effort at reduction unavailing, I was tempted to destroy it by inducing a state of faint by bleeding, before I attempted the reposition. This succeeded, and has done so in every instance in which I have tried it. I therefore recommend it with confidence, convinced that in some instances it is the only means we have in our power to do away with this kind of difficulty. It may not always be necessary to employ bleeding, perhaps, to induce this disposition to syncope; it may be done by other means, probably with equal advantage, such as tartar emetic by the mouth or the infusion of tobacco by the rectum; but of neither have I had any experience.

But before we bleed *ad deliquium*, we should have every thing arranged for the operation of reposition, that no delay may be created after this state is induced; we should have previously evacuated the rectum and have emptied the bladder; the bed should be prepared in such a manner as will allow the patient to lay upon her back, with the perinæum free over the edge of the bedstead, and her shoulders depressed lower than the hips; care should be taken to have a matrass or some other firm and protecting substance between the back of the woman and the bedstead—the parts should be well lubricated—a chair should be placed for each foot, and the legs must be supported by an attendant for each. When every thing is thus prepared, the patient should be placed on her feet near the part of the bed prepared for her reception, her arm tied up, and from a large orifice subtract as much blood as will produce the desired state of faintness; so soon as this is observed the arm should be tied up, and the patient placed as directed above; the hand well lubricated, should then be gradually introduced into the vagina in a state of supination; the fingers retracted in such manner as make them form a straight line at their extremities; these must now be gently pressed against the most depending and posterior portion of the tumour that is found within the vagina, and carry it backwards and upwards along the hollow of the sacrum, until the mass shall reach above the projection of this bone; when arrived here we may withdraw the hand, introduce a sufficiently large pessary, and direct the woman to remain quietly in bed, after having been placed there, for three or four days. We should, for this period at least, draw off the urine by the catheter at least twice in twenty-four hours; and the bowels emptied by mild injections. This plan has succeeded, where I am persuaded I should have failed without it.

The last difficulty we have to encounter is where the size of the uterus is larger or as large as the opening of the superior strait. This will immediately present itself as one of immense consequence and difficulty—the lives of both mother and child are at stake—but perhaps one not beyond the resources of our art. I am disposed to believe that, where the size of the uterus is not larger than the opening of the superior strait, that we may sometimes succeed by employing the plan just suggested. It should at all events be tried before severer measures are resorted to. If it fail, we certainly do not lose by having made the trial. But what is to be done should it fail? This is a question of great moment, and may be resolved by adopting one of three modes of practice.

First, to confide entirely in the resources of nature, as recommended by Merriman.

Second, to attempt the production of abortion, by breaking the membranes through the os tincæ.

Third, to puncture the uterus through the rectum or vagina, with the same intention, as recommended by Dr. Hunter.

As to the first, we have, from all we can learn, but little temptation to adopt it; and I am of opinion it never should be but as a dernier resource. The second, if practicable, would unquestionably be the mildest and safest; but we are led to believe that it will not always, nor indeed perhaps never, be practicable; but it should first be essayed, before we have recourse to the third. I taught this to my pupils four and twenty years ago, and I thought until within a few days it was an original suggestion; but in reading the case related by Mr. White, I find it was attempted by him at the request of Mr. Hamilton, afterwards professor Hamilton, under similar circumstances, but without success. It has lately been attempted by M. Jourel,* who says, in his journal of a very interesting case, which we shall in part relate, "Le soir du même jour, tentative infructueuse pour introduire un cathéter par l'orifice de la matrice afin de crever les membranes et de donner issue aux eaux de l'amnios: le col de l'organe, trop fortement courbé, s'opposa à cette manœuvre dont nul auteur n'avait parlé." It would seem that M. Jourel had not met with the case of Mr. White, wherein this operation is recommended, nor the reviewer of his case, who agrees to call it an unprecedented manœuvre. I am, however, still of opinion, that a flexible catheter might perhaps succeed, if cautiously conducted with this view.

The third alternative, has been universally condemned by the British writers, but it would seem, without sufficient ground; for it has actually succeeded in the hands of M. Jourel,* in a recent trial in a case of retroversion. As this case is highly interesting and but little known, I shall take the liberty of condensing and relating it.

"A woman aged 23 years, was at the period of six weeks of pregnancy rudely handled in a frolic; she was soon after seized with an hæmorrhagy from the uterus, accompanied with pains in the back and groins, with a sensation of weight in the perinæum, a difficulty in walking and voiding her fæces. M. Jourel was at the end of a fortnight consulted; he recommended rest, and the use of some astringent drinks. The woman went into the country, and was not seen for a month by M. Jourel. At the end of this time she told him that the discharge from the vagina had ceased within two days; but that all her other symptoms had increased, so that she voided with great difficulty both urine and fæces. Relief was attempted by the catheter and injections. Six days after her return, an examination was made per vaginam by M. Jourel and one of his friends, and the uterus was found in a state of retroversion. An attempt was made at reposition, without success; this was repeated on the next day with a similar result—they now attempted to produce abortion by introducing a catheter into the mouth of the uterus, but the neck of this viscus was so bent, as to prevent the operation from succeeding. It was then agreed to adopt Dr. Hunter's method of puncturing the uterus, in preference to Gardien's operation of pubic synchondrotomy. M. Jourel operated with a common trocar passed along the fore finger of the left hand, so as to pierce the posterior part of the vagina. The canula transmitted about a pound of bloody fluid; the uterus immediately became softer, the pulse less frequent, and the general condition of the woman was improved. As the woman was much fatigued, immediate reduction was not attempted.

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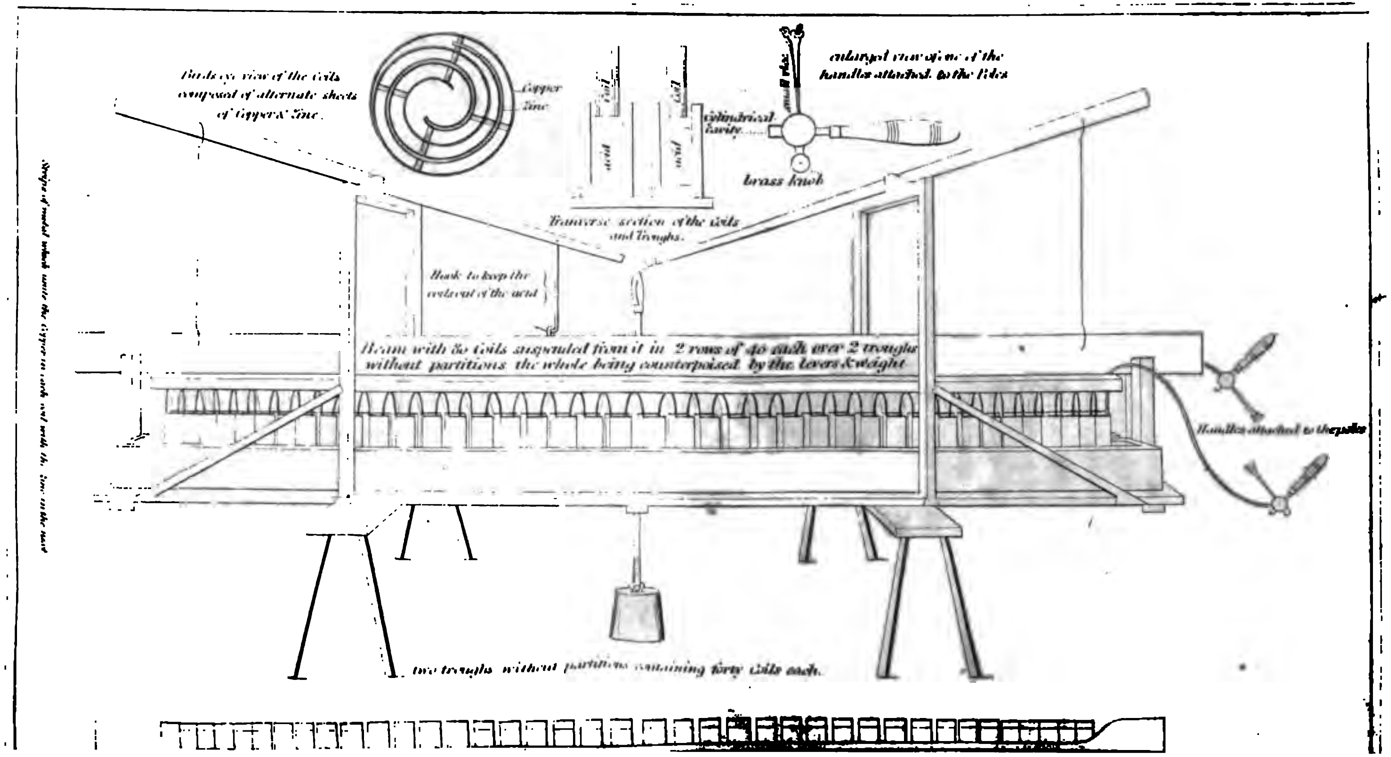
* Dictionnaire des Sciences Médicales, art. Deviation, vol. ix. p. 81.

† Ibid. loc. cit.

"From this time the urine was freely evacuated; the next day things continued in the same state—much serum oozed from the vagina—the uterus became a little painful, and narcotic injections and fomentations were prescribed for its relief. After this the pulse became smaller and more frequent; the belly was sore to the touch; the uterus harder and more tender. The difficulty of discharging urine returned in the forenoon; she vomited twice, and had discharges of flatus per anum. Her strength failed; the discharge was interrupted, but returned the next day; her stools thin. Towards evening the symptoms became milder, and continued to subside, but she remained still very weak. She voided her urine partly involuntarily.

"Typhoid symptoms began to shew themselves, and there was a putrid discharge from the vagina—a copious and involuntary discharge of urine when in a vertical situation. Bark injections were employed. By the use of tonics her strength was improved; the fœtid discharge only took place at intervals. On the 27th of September, thirteen days after the operation, the uterus restored itself to its natural position. On the 2d of October it was reduced to its usual size.

GALVANIC DEFLAGRATOR.


Birds eye view of the Coils composed of alternate sheets of Copper & Zinc.
Copper
Zinc
coil
coil
acid
acid
cylindrical Cavity
small tube
enlarged view of one of the handles attached to the Poles
brass knob
Transverse section of the Coils and Troughs.
Hook to keep the reservoir of the acid
Beam with 80 Coils suspended from it in 2 rows of 40 each over 2 troughs without partitions the whole being counterpoised by the levers & weight
Handles attached to the poles
two troughs without partitions containing forty Coils each.
Strips of metal which unite the Copper in each coil with the Zinc in the next

There was a discharge of a puriform substance from the anus, which gradually subsided, and ceased altogether on the 10th of the same month. The patient went into the country for three weeks; during this interval, and till the 15th of December, when menstruation was restored, she experienced a distressing tightness of the abdomen, and some colicky pains; but after the restoration of the catamenial discharge, she enjoyed good health."

The puncture of the bladder above the pubes, has also been performed with success in a case of retroversion, by Dr. Cheston.* In this case the bladder was much distended, even above the navel; the water was several times drawn off by the catheter, but at length a total suppression took place, and though a male catheter could be introduced two thirds of its length, the bladder could not be emptied—the attempts at reduction failed; the woman was about four months advanced in pregnancy—there appeared no other alternative but puncturing the bladder. This was accordingly done above the symphysis pubis, and the water evacuated; the uterus restored itself, and the woman went her full time, was safely delivered, and has since been pregnant. Dr. Cheston suggests two cautions in performing this operation: one that the urine should be drawn off very gradually indeed, and the other that a short trocar should be used—he mentions a fatal case, where a long one was employed.

Art. III. *A Memoir on some New Modifications of Galvanic Apparatus, with Observations in Support of his New Theory of Galvanism.* By R. Hare, M. D. Professor of Chemistry in the University of Pennsylvania.

I HAD observed that the ignition produced by one or two galvanic pairs attained its highest intensity, almost as soon as they were covered by the acid used to excite them, and ceased soon afterwards; although the action of the acid should have increased during the interim. I had also remarked in using an apparatus of three hundred pairs of small plates, that a platina wire number sixteen placed in the circuit, was fused in consequence of a construction which enabled me to plunge them all nearly at the same time. It was therefore conceived, that the maximum of effect in voltaic apparatus of extensive series had never been attained. The plates are generally arranged in distinct troughs rarely containing more than twenty pairs. Those of the great apparatus of the Royal Institution, employed by Sir H. Davy, had only ten pairs in each. There were one hundred such to be successively placed in the acid, and the whole connected ere the poles could act. Consequently the effect which arises immediately after immersion, would be lost in the troughs first arranged, before it could be produced in the last; and no effort appears to have been made to take advantage of this transient accumulation of power, either in using that magnificent combination, or in any other of which I have read. In order to observe the consequence of simultaneous immersion with a series sufficiently numerous to test the correctness of my expectations, a galvanic apparatus of eighty concentric coils of copper and zinc, was so suspended by a beam and levers, as that they might be made to descend into, or rise out of the acid in an instant. The zinc sheets were about nine inches by six, the copper fourteen by six; more of this metal being necessary, as in every coil it was made to commence within the zinc, and completely to surround it without. The sheets were coiled so as not to leave between them an interstice wider than a quarter of an inch. Each coil is in diameter about two inches and a half, so that all may descend freely into eighty glass jars two inches and three quarters diameter inside, and eight inches high, duly stationed to receive them. See plate.

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* Med. Communications, vol. ii. p. 6.

My apparatus being thus arranged, two small lead pipes were severally soldered to each pole, and a piece of charcoal about a quarter of an inch thick, and an inch and a half long, tapering a little at each extremity, had these severally inserted into the hollow ends of the pipes: The jars being furnished with diluted acid and the coils suddenly lowered into them, no vestige of the charcoal could be seen: It was ignited so intensely, that those portions of the pipes by which it had been embraced were destroyed. In order to avoid a useless and tiresome repetition, I will here state that the coils were only kept in the acid while the action at the poles was at a maximum in the experiment just mentioned, and in others which I am about to describe, unless where the decomposition produced by water is spoken of, or the sensation excited in the hands. I designate the apparatus with which I performed them, as the galvanic deflagrator, on account of its superior power, in proportion to its size, in causing deflagration; and as, in the form last adopted, it differs from the voltaic pile in the omission of one of the elements heretofore deemed necessary to its construction.

Desirous of seeing the effect of the simultaneous immersion of my series upon water, the pipes soldered to the poles were introduced into a vessel containing that fluid. No extraordinary effect was perceived, until they were very near, when a vivid flash was observed, and happening to touch almost at the same time, they were found fused and incorporated at the place of contact. I next soldered to each pipe a brass cylinder about five-tenths of an inch bore. These cylinders were made to receive the tapering extremities of a piece of charcoal about two inches long so as to complete the circuit. The submersion of the coils caused the most vivid ignition in the coal. It was instantaneously and entirely on fire. A piece of platina of about a quarter of an inch diameter in connexion with one pole, was instantly fused at the end on being brought in contact with some mercury communicating with the other. When two cylinders of charcoal having hemispherical terminations were fitted into the brass cylinders and brought nearly into contact, a most vivid ignition took place, and continued after they were removed about a half or three-quarters of

an inch apart, the interval rivalling the sun in brilliancy. The igneous fluid appeared to proceed from the positive side. T charcoal in the cylinder soldered to the latter would be intensely ignited throughout when the piece connected with the negative pole was ignited more towards the extremity approaching the positive. The most intense action seems to arise from placing a platina wire of about the eighth of an inch diameter, in connexion with the positive pole, a
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* According to the theory here alluded to, the galvanic fluid owes its properties to caloric and electricity; the former predominating in proportion to the size of the pairs, the latter in proportion to the number, being in both cases excited by a powerful acid. Hence in batteries which combine both qualifications sufficiently, as in all those intervening between Children's large pairs of two feet eight inches by six feet, and the 2000 four-inch pairs of the Royal Institution, the phenomena indicate the presence of both fluids. In De Luc's column, where the size of the pairs is insignificant, and the energy of interposed agents feeble, we see electricity evolved without any appreciable quantity of caloric. In the calorimotor where we have size only, the number being the lowest possible, we are scarcely able to detect the presence of electricity.

When the fluid contains enough electricity to give a projectile power adequate to pass through a small space in the air, or through charcoal, which impedes or arrests the caloric, and favours its propensity to radiate, this principle heat is evolved. This accounts for the evolution of intense heat under those circumstances which rarifies the air, so that the length of the jet from one pole to the other may be extended after its commencement. Hence the portions of the circuit nearest to the intervening charcoal, or heated space, are alone injured; and even non-conducting bodies, as quartz, introduced into it are fused, and hence a very large wire may be melted by the fluid, received through a small wire imperceptibly affected.

See Silliman's Journal, No. 6, Vol. 1. Thomson's Annals, Sept. 1810. Tilloch' Philosophical Magazine, October, 1819.

the product of the most powerful machine without detriment, if received gradually as generated by them. Platina points, as small as those which were melted like wax in my experiments, are used as tips to lightening rods without injury, unless in sudden discharges produced under peculiar circumstances.*

The following experiment I conceive to be very unfavourable to the idea that galvanic ignition arises from a current of electricity.

A cylinder of lead of about a quarter of an inch diameter, and about two inches long, was reduced to the thickness of a common brass pin for about three quarters of an inch. When one end was connected with one pole of the apparatus, the other remained suspended by this filament; yet it was instantaneously fused by contact with the other pole. As all the calorific fluid which acted upon the suspended knob, must have passed through the filament by which it hung, the fusion could not have resulted from a pure electrical current, which would have dispersed the filament ere a mass fifty times larger had been perceptibly affected. According to my theory, caloric is not separated from the electricity until circumstances very much favour a disunion, as on the passage of the compound fluid through charcoal, the air, or a vacuum. In operating with the deflagrator, I have found a brass knob of about five tenths of an inch in diameter, to burn on the superficies only; where alone according to my view, caloric is separated so as to act on the mass. Having, as mentioned in the memoir on my theory of galvanism, found that four galvanic surfaces acted well in one recipient, I was tempted by means of the eighty coils to extend that construction. It occurred to me that attempts of this kind, had failed from using only one copper for each zinc plate. The zinc had always been permitted to react towards the negative, as well as the positive pole. My coils being surrounded by copper, it seemed probable, that, if electro-calorie were, as I had suggested, carried forward by circulation arising from galvanic polarity, this might act within the interior of the coils, yet not be exerted between one coil and another.

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* See Adams's Electricity, On points.

I had accordingly a trough constructed with a partition along the middle, so as to receive forty coils on one side, and a like number on the other. This apparatus when in operation excited a sensation scarcely tolerable in the backs of the hands. Interposed charcoal was not ignited as easily as before, but a most intense ignition took place on bringing a metallic point connected with one pole of the series, into contact with a piece of charcoal fastened to the other. It did not take place however so speedily as when glasses were used; but soon after the ignition was effected it became even more powerful than before. A cylinder of platina nearly a quarter of an inch in diameter, tapering a little at the end, was fused and burned so as to sparkle to a considerable distance around, and fall in drops. A ball of brass of about half an inch diameter was seen to burn on its surface with a green flame. Tin foil, or tinsel rolled up into large coils of about three quarters of an inch thick, were rapidly destroyed, as was a wire of platina of No. 16. Platina wires in connexion with the poles were brought into contact with sulphuric acid; there was an appearance of lively ignition, but strongest on the positive side. Excepting in its power of permeating charcoal, the galvanic fluid seemed to be extricated with as much force, as when each coil was in a distinct glass. Apprehending that the partition in the trough did not sufficiently insulate the poles from each other, as they were but a few inches apart, moisture or moistened wood intervening, I had two troughs each to hold forty pairs, and took care that there should be a dry space about four inches broad between them. They were first filled with pure river water, there being no saline nor acid matter to influence the plates, unless the very minute quantity which might have remained on them from former immersions. Yet the sensation produced by them, on the backs of my hands, was painful; and a lively scintillation took place when the poles were approximated. Dutch gold leaf was not sensibly burned, though water was found decomposible by wires properly affixed. No effect was produced on potash, the heat being inadequate to fuse it.

A mixture of nitre and sulphuric acid was next added to the water in the troughs, afterwards charcoal from the fire was vividly ignited, and when attached to the positive pole a steel wire was interposed between it and the other pole, the most vivid ignition which I ever saw was induced. I should deem it imprudent to repeat the experiment without glasses, as my eyes, though unusually strong, were affected for forty-eight hours afterwards. If the intensity of the light did not produce an optical deception, by its distressing influence upon the organs of vision, the charcoal assumed a pasty consistence, as if in a state approaching to fusion. —That charcoal should be thus softened, without being destroyed by the oxygen of the atmosphere, will not appear strange, when the power of galvanism in reversing chemical affinities is remembered; and were it otherwise, the air could have no access, first, because of the excessive rarefaction, and in the next place as I suspect on account of the volatilization of the carbon forming about it a circumambient atmosphere. This last mentioned impression arose from observing, that when the experiment was performed in vacuo, there was a lively scintillation, as if the carbon in an aeriform state acted as a supporter of combustion on the metal.

A wire of platina (No. 16) was fused into a globule on being connected with the positive pole, and brought into contact with a piece of pure hydrate of potash, situated on a silver tray in connexion with the other pole. The potash became red hot, and was deflagrated rapidly with a flame having the rosy hue of potassuretted hydrogen.

The great apparatus of the Royal Institution, *in projectile power* was from six to eight times more potent than mine. It produced a discharge between charcoal points when removed about four inches apart, whereas mine will not produce a jet at more than three fourths of an inch. But that series was 2000, mine only about a twenty-fifth part as large.

A steel wire of about one tenth of an inch in diameter, affixed to the negative pole, was passed up through the axis of an open necked inverted bell glass, filled with water. A platina wire, No. 16, attached to a positive pole being passed down to the steel wire, both were fused together, and cooling, could not be separated by manual force.—Immediately after this incorporation of their extremities, the platina wire became incandescent for a space of some inches above the surface of the water.

A piece of silvered paper about two inches square was folded up, the metallic surface outward, and fastened into vices affixed to the poles. Into each vice a wire was screwed at the same time. The fluid generated by the apparatus was not perceptibly conveyed by the silvered paper, as it did not prevent the wires severally attached to the poles from decomposing water or producing ignition by contact.

In my memoir on my theory of galvanism I suggested, that the decomposition of water, which Wollaston effected by mechanical electricity, might not be the effect of divellent attraction like those excited by the poles of a voltaic pile, but of a mechanical concussion, as when wires are dispersed by the discharge of an electrical battery. In support of that opinion I will now observe, that he could not prevent hydrogen and oxygen from being extricated at each wire, instead of hydrogen being given off only at one, and oxygen at the other, as is invariably the case when the voltaic pile is employed. That learned and ingenious philosopher, in concluding his account of this celebrated experiment, says " but in fact the resemblance is not complete, for in every way in which I have tried it, I observed each wire gave out both oxygen and hydrogen gas, instead of their being formed separately as by the electric pile."

Is it not reasonable to suppose that an electrical shock may dissipate any body into its elementary atoms, whether simple or compound, so that no two particles would be left together which can be separated by physical means.

Looking over Singer's Electricity, a recent and most able modern publication, I find that in the explosion of brass wire by an electrical battery, the copper and zinc actually separated. He says, page 186, "Brass wire is sometimes *decomposed* by the charge; the copper and zinc of which it is formed being separated from each other, and appearing in their distinct metallic colours." On the next page in the same work, I find that the oxides of mercury and tin are reduced by electrical discharges. "Introduce," says the author, "some oxide of tin into a glass tube, so that when the tube is laid horizontal, the oxide may cover about half an inch of its lower internal surface. Place the tube on the table of the universal discharger, and introduce the pointed wires into its opposite ends, that the portion of oxide may lay between them. Pass several strong charges in succession through the tube, replacing the oxide in its situation, should it be *dispersed*. If the charges are sufficiently powerful, a part of the tube will soon be stained with metallic tin which has been revived by the action of transmitted electricity." It cannot be alleged that in such decompositions the divellent polar attractions are exercised like those which characterize the action of wire proceeding from the poles of a voltaic apparatus. The particles were dispersed from, instead of being attracted to the wires, by which the influence was conveyed among them. This being undeniable, it can hardly be advanced that we are to have one mode of explaining the separation of the elements of brass by an electrical discharge, another of explaining the separation of the elements of water by the same agent. One rationale when oxygen is liberated from tin, and another when liberated by like means from hydrogen. In the experiment in which copper was precipitated by the same philosopher at the negative pole, we are not informed whether the oxygen and acid in union with it were attracted to the other; and the changes produced in litmus are mentioned not as simultaneous, but successive. The violet and red rays of the spectrum have an opposite chemical influence in some degree like that of voltaic poles, but this has not led to the conclusion that the cause of galvanism and light is the same. Besides admitting that the feeble results obtained by Wollaston and Van Marum are perfectly analogous to those obtained by the galvanic fluid, ere it can become an objection to my hypothesis, it ought first to be shown that the union between caloric and electricity, which I suppose productive of galvanic phenomena, cannot be produced by that very process. If they combine to form the galvanic fluid when extricated by ordinary galvanic action, they must have an affinity for each other. As I have suggested in my memoir, when electricity enters the pores of a metal it may unite with its caloric. In Wollaston's experiments, being constrained to enter the metal, it may combine with enough of its caloric to produce, when emitted, results slightly approaching to those of a fluid in which caloric exists in greater proportion.

But once more I demand why, if mechanical electricity be too intense to produce galvanic phenomena, should it be rendered more capable of producing them by being still more concentrated.

If the one be generated more copiously, the other more intensely, the first will move in a large stream slowly, the last in a small stream rapidly. Yet by narrowing the channel of the latter, Wollaston is supposed to render it more like the former, that is, produces a resemblance by increasing the supposed source of dissimilarity.

It has been imagined that the beneficial effect of his contrivance arises from the production of a continued stream, instead of a succession of sparks, but if a continued stream were the only desideratum, a point placed near the conductor of a powerful machine would afford this requisite, as the whole product may in such cases be conveyed by a sewing needle in a stream perfectly continuous. As yet no adequate reasons have been given why, in operating with the pile, it is not necessary, as in the processes of Van Marum and Wollaston, to enclose the wires in glass or sealing wax, in order to make the electricity emanate from a point within a conducting fluid. The absence of this necessity is accounted for, according to my hypothesis by the indisposition which the electric fluid has to quit the caloric in union with it, and the almost absolute incapacity which caloric has to pass through fluids unless by circulation. I conceive that in galvanic combinations, electro-caloric may circulate through the fluid from the positive to the negative surface, and through the metal from the negative to the positive. In the one case caloric subdues the disposition which electricity has to diffuse itself through fluids, and carries it into circulation. In the other, as metals are excellent conductors of caloric, the prodigious power which electricity has to pervade them agreeably to any attractions which it may excercise, operates almost without restraint. This is fully exemplified in my galvanic deflagrator, where eighty pairs are suspended in two recipients, forty successively in each, and yet decompose potash with the utmost rapidity, and produce an almost intolerable sensation* when excited only by fresh river water. I have already observed that the reason why galvanic apparatus composed of pairs consisting each of one copper and one zinc plate, have not acted well without insulation;† was because electro-caloric could retrocede in the negative, as well as advance in the positive direction. I will now add, that independently of the greater effect produced by the simultaneous immersion of my eighty coils, their power is improved by the proximity of the surfaces, which are only about an eighth of an inch asunder; so that the circulation may go on more rapidly.

Pursuant to the doctrine, which supposes the same quantity of electricity, varying in intensity in the ratio of the number of pairs to the quantity of surface, to be the sole agent in galvanic ignition, the electrical fluid as evolved by Sir H. Davy's great pile, must have been nearly two thousand times more intense, than as evolved by a single pair, yet it gave sparks at no greater distance than the thirtieth or fortieth of an inch. The intensity of the fluid must be at least as much greater in one instance, than in another, as the sparks produced by it are longer. A fine electrical plate machine of thirty-two inches diameter, will give sparks at ten inches. Of course the intensity of the fluid which it emits, must be three hundred times greater than that emitted by two thousand pairs. The intensity produced by a single pair, must be two thousand times less than that produced by the great pile, and of course six hundred thousand times less than that produced by a good electrical plate of thirty-two inches. Yet a single pair of about a square foot in area, will certainly deflagrate more wire, than a like extent of coated surface charged by such a plate. According to Singer, it requires about one hundred and sixty square inches of coated glass, to destroy watch pendulum wire; a larger wire may be burned off by a galvanic battery of a foot square. But agreeably to the hypothesis in dispute, it compensates by quantity, for the want of intensity. Hence the quantity of fluid in the pair is six hundred thousand times greater, while its intensity is six hundred thousand times less; and *vice versa* of the coated surface. Is not this absurd? What does intensity mean as applied to a fluid? Is it not expressed by the ratio of quantity, to space? If there be twice as much electricity within one cubic inch, as within another, is there not twice the intensity? But the one acts suddenly, it may be said; the other slowly. But whence this difference? They may both have exactly the same surface to exist in. The same zinc and copper plates may be used for coatings first, and a galvanic pair afterwards. Let it be said, as it may in truth, that the charge is, in the one case, attached to the glass superficies, in the other exists in the pores of the metal. But why does it avoid these pores in one case and reside in them in the other? What else resides in the pores of the metal which may be forced out by percussion? Is it not caloric? Possibly, unless under constraint, or circumstances favourable to a union between this principle and electricity, the latter cannot enter the metallic pores, beyond a certain degree of saturation; and hence an electrical charge does not reside in the metallic coatings of a Leyden phial, though it fuses the wire which forms a circuit between them.

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* I do not say shock, as it is more like the permanent impression of hot pointed wire, especially when an acid is used.

† That is, with the same mass of conducting fluid, in contact with all the surfaces, instead of being divided into different portions, each restricted in its action to one copper and one zinc plate.

It is admitted that the action of the galvanic fluid, is upon or between atoms; while mechanical electricity when uncoerced, acts only upon masses. This difference has not been explained unless by my hypothesis, in which caloric, of which the influence is only exerted between atoms, is supposed to be a principal agent in galvanism. Nor has any other reason been given that water, which dissipates pure electricity, should cause the galvanic fluid to accumulate. From the prodigious effect which moist air, or a moist surface, has in paralysing the most efficient machines, I am led to suppose, that the conducting power of moisture so situated, is greater than that of water under its surface. The power of this fluid to conduct mechanical electricity, is unfairly contrasted with that of a metal, when the former is enclosed in a glass tube, the latter bare.

According to Singer, the electrical accumulation is as great when water is used, as when more powerful menstrua are employed; but the power of ignition is wanting, until these are resorted to. De Luc showed, by his ingenious dissections of the pile, that electricity might be produced *without*, or *with* chemical power. The rationale of these differences never has been given, unless by my theory, which supposes caloric to be present in the one case, but not in the other. The electric column was the fruit of De Luc's sagacious inquiries, and afforded a beautiful and incontrovertible support to the objections he made to the idea, that the galvanic fluid is pure electricity, when extricated by the voltaic pile in its usual form. It showed that a pile really producing pure electricity, is devoid of the chemical power of galvanism.

We are informed by Sir H. Davy, that when charcoal points, in connection with the poles of the magnificent apparatus with which he operated, were first brought nearly into contact, and then withdrawn four inches apart, there was a heated arch formed between them, in which such non-conducting substances as quartz were fused. I believe it impossible to fuse electrics by mechanical electricity. If opposing its passage they may be broken, and if conductors near them be ignited, they may be acted on by those ignited conductors as if otherwise heated; but I will venture to predict, that the slightest glass fibre will not enter into fusion, by being placed in a current from the largest machine or electrical battery.

I am induced to believe, that we must consider light, as well as heat, an ingredient in the galvanic fluid; and think it possible, that, being necessary to vitality in animals, as well as vegetables, the electric fluid may be the vehicle of its distribution.

I will take this opportunity of stating, that the heat evolved by one galvanic pair has been found by the experiments which I instituted, to increase in quantity, but to diminish in intensity, as the size of the surfaces may be enlarged. A pair containing about fifty square feet of each metal, will not fuse platina, nor deflagrate iron, however small may be the wire employed; for the heat produced in metallic wires is not improved by a reduction in their size beyond a certain point. Yet the metals abovementioned, are easily fused or deflagrated by smaller pairs, which would have no perceptible influence on masses that might be sensibly ignited by larger pairs.—These characteristics were fully demonstrated, not only by my own apparatus, but by those constructed by Messrs. Wetherill and Peale, and which were larger, but less capable of exciting intense ignition. Mr. Peale's apparatus contained nearly seventy square feet, Mr. Wetherill's nearly one hundred, in the form of concentric coils, yet neither could produce a heat above redness on the smallest wires. At my suggestion, Mr. Peale separated the two surfaces in his coils into four alternating, constituting two galvanic pairs in one recipient. Iron wire was then easily burned and platina fused by it. These facts, together with the incapacity [illegible] the calorific fluid extricated by the calorimotor to permeate charcoal, next to metals the best electrical conductor, must sanction the position I assigned to it as being in the opposite extreme from the columns of De Luc and Zamboni. For as in these, the phenomena are such as are characteristic of pure electricity, so in one very large galvanic pair, they almost exclusively demonstrate the agency of pure caloric.

Art. IV. *Observations and Experiments on certain Parts of the Nervous System.* By William E. Horner, M. D.

It is not a little remarkable, that the numberless inquiries into the functions of the nervous system, have terminated in results of such contradictory characters, and afforded so few points on which a general coincidence of sentiment prevails. Being called upon within a few years to assist two graduates in this school in a course of experiments on the brain and spinal marrow, I had reason to believe, from what I then saw, that the common opinion of the uses of these parts was so mixed with error, as to require a revision. This investigation was undertaken with no preconceived notions, or attachment to any established system. Conducted only by an honest desire of arriving at truth, I have a degree of confidence in the correctness of the conclusions from my numerous and concurrent experiments, which I should not have felt or admitted under other circumstances.

The nervous system, in the mammalia, may with propriety be considered under two divisions. The one consists of the senses of seeing, hearing, smelling, &c., constituting the organs of external life, or that by which we live in relation to surrounding objects, and the other comprises that arrangement of the nerves by which the different parts of the same body are held in a sympathetic connexion and their functions sustained. It is obvious that the first section embraces organs merely accessory to the life of the individual, which improve his condition, though, being by no means directly essential to his existence, may be destroyed without death, while the latter includes a set of nerves immediately and indissolubly connected with life.

One of the most striking circumstances in the nervous system is the variety which it exhibits in the different classes of animals. In man and the mammalia it consists of a cerebrum, a cerebellum [illegible] varolii, medulla oblongata, and medulla [illegible] m these, numerous cords pass to all parts [illegible] he brain in man is much greater in proporti [illegible] inal marrow than in any other animal, and th [illegible] ow predominates much in its thickness o [illegible] hich proceed from it. In some fish [illegible] eeds in size the medulla oblo [illegible] e is only a brain, which send [illegible] a centre, to ganglions scattered o [illegible] t as large as the brain itself. In ins [illegible] s scarcely larger than one of the numerous swellings of the spinal marrow, and sends off its nerves in the same way. In polypi there is no regular and distinct nervous system, and hence it appears the lower we descend in the scale of animals, the less this system is to be found concentrated in a particular region and the more it is diffused over the whole body.

This general view of the condition of the nervous system in different animals, proves the necessity of its existing in order that animal life may be possessed: and it also exhibits another important feature, that it is susceptible of a great variety of exterior modifications without having its intrinsic functions impaired. It is observed in regard to this system, that the more it is accumulated in centers, the more important its integrity becomes to the life of the whole; for example, a toad or a tortoise has comparatively a small brain, which exercises so slight an influence on the general condition of the animal, that its removal will not prevent life from being sustained in its usual relations and vigour for many months afterwards. There are many species of worms and insects, which on being cut into two or more parts, each of those parts becomes an independent animal, possessing its own sensations and distinct powers of life: and many of the zoophytes on being divided almost to infinity, from the general diffusion of the nervous principle in them, have a distinct existence communicated to each section. It is only when we approach the higher orders of animals, that the integrity of the nervous system or the presence of its central parts, becomes indispensable to life, and this necessity of their presence is more strongly perceived, the larger that the brain is in proportion to its ramifications.

The foregoing facts render it probable that the nervous system is homogeneous in all animals, and indeed we may conclude it to be the same in all parts of the same animal, from the metastasis of function which occasionally has occurred where a sense has been lost either naturally or by accident, and therefore that all the varieties of its functions depend upon the peculiar nature and arrangement of the organ supplied with nervous matter. We are also furnished with another idea from examining the state of the nervous system in different animals, namely, that, as in polypi, &c., a new animal can be generated from a section of an old one, so it is evident that each part possesses integral properties, and consequently must have the principles or modifications of its nervous system all blended together, whereas in man and all other mammalia these modifications have each its particular throne or habitation, as the eye that by which we see, the tongue that by which we taste, and so on of the other senses.

We find, moreover, by experiment, that other locations of functions take place in the latter animals, that the brain is the seat of intelligence, the medulla oblongata of the principle of respiration, and that the medulla spinalis is the immediate seat of life to all other parts. Be it allowed that such localities of vital functions do exist, of which there seems to me to be so little doubt, why may not the different parts of the brain afford uniform position to the moral faculties?

It has been considered probable, that nearly all of the nerves find an increase of matter as they get further from their origin in the brain or spinal marrow, which accounts for the sensibility met with every where on the surface of the body, so diffused and minute in its division, that the finest needle cannot be introduced without causing pain. Though it is true that the opinion of the extreme terminations of nerves being larger or containing more matter in the aggregate than the trunks from which they emanate, is opposed by the common appearances on dissection, for there we find that as ramifications pass off the nerve is diminished, still it is difficult to explain the universal sensibility of different organs without supposing some such arrangement. The fibres of muscles, the surface of the skin, and other sensitive parts, may possibly, then, receive something like a coating of nervous matter; and if this be the case, the medulla derived from the brain and spine is sufficient to furnish the whole body, as it may possess a plasticity and ductility somewhat like that of gold, which the natural philosophers tell us are so great, that a grain of the metal may be beaten out to cover a surface of several feet square, and one cubic inch is sufficient to gild completely a wire long enough to surround the globe.

The celebrated Hunter, in his view of the vitality of the blood, admits unequivocally the general diffusion of a nervous matter through the whole system, though he does not undertake to describe the mode of arrangement. His words are, "I consider that something similar to the materials of the brain is diffused through the body, and even contained in the blood: between this and the brain a communication is kept up through the nerves; I have therefore adopted terms explanatory of this theory; calling the brain the *materia vitæ coascervata*, the nerves the *chordæ internunciæ*, and that diffused through the body the *materia vitæ diffusa*. Of this latter every part of an animal has its portion. It is, as it were, diffused through the whole solids and fluids, making a necessary constituent part of them, and forming with them a perfect whole; giving to both the power of preservation, the susceptibility of impression, and from their construction, giving them consequent reciprocal action. This is the matter which principally composes the brain; and where there is a brain, there must necessarily be parts to connect it with the rest of the body, which are the nerves; and the use of the nerves is to continue, and therefore convey, the impression or action from one to the other. These parts of communication must necessarily be of the same matter; for any other matter could not continue the same action.

"From this it may be understood, that nothing material is conveyed from the brain by the nerves, nor, *vice versa*, from the body to the brain: for if that was exactly the case, it would not be necessary for the nerves to be of the same materials with the brain; but as we find the nerves of the same materials, it is a presumptive proof that they only continue the same action which they receive at either end."

Conceding this MATERIA VITÆ DIFFUSA to be a coating of nervous matter over the whole fibrous structure of the body, we can, from its degree of profusion, estimate, and even anticipate, the sensibility of different organs, this quality being small when the matter is sparingly diffused, and highly exquisite when it is abundant. In this view of the subject we see the reason for the accumulation of the pulpy nervous matter of the retina, the materia vitæ diffusa having to appear there in the form of a continuous membrane, in order that a delicacy of sensibility might be given to the organ capable of receiving an impression so inconceivably feeble as that occasioned by the matter of light. This latter substance, travelling at the immense velocity of two hundred thousand miles in one second, though it has been concentrated in focus upon focus, with a view of determining whether in this condensed condition it possesses any appreciable momentum, has always escaped the test of the most delicate instruments. An organ of this exquisitely refined sensibility is alone capable of responding to a stimulus so exceedingly subtile that it is even questioned whether it is a portion, or merely a quality of matter.

The structure and expansion of the auditory nerve, a pulpy distribution on the internal surface of the labyrinth, resembling the retina, affords a parallel, in some measure, to the optic, depending for the performance of its peculiar functions on a degree of sensibility capable of feeling the most delicate vibrations of the air.

We do not deem it important to the object of our present paper, to enter further into the investigation whether the nervous principle is supplied to each molecule of the body or not, the fact that each part has sensibility being sufficient. But the question is, whether this sensibility be inherent in every part of the human frame, or extraneous, and its origin to be looked for elsewhere. The following experiments will, I think, prove the latter hypothesis.

As it was Mons. Le Gallois's work on the principle of life, which incited me to the pursuit of this interesting inquiry, I shall assume it as a model for my experiments as well as follow his division of the subject, and therefore commence with laying down the following positions:

First, That the principle of all inspiratory motions resides in that part of the medulla oblongata which gives rise to the nerves of the eighth pair.

Second, That the principle which animates every part of the body, resides in that part of the medulla spinalis, from which the nerves of that part originate.

Third, That it is likewise from the medulla spinalis, that the heart receives the principle of its life and of its power, but in the whole medulla, however, and not in a circumscribed portion of it.

Experiment 1*st.*—Took a kitten four days old, and divided the spinal marrow between the occipital foramen and first cervical vertebra, which instantly stopped respiration. The animal was much agitated, and gaped frequently. At the end of ten minutes, when sensibility had almost ceased, the larynx was divided from the os hyoides, and the lungs artificially inflated. By continuing the inflation for five minutes, the animal was evidently much revived. At the end of twenty-five minutes (sensation and the power of motion continuing) the spine was divided between the tenth and eleventh dorsal vertebræ. At thirty minutes each of the parts thus separated retained sensibility and motion: but all sympathy between them was destroyed, as an impression made upon the fore parts produced no effect upon the hinder, and *vice versa*. At forty minutes, the posterior extremities were insensible: at forty-five minutes the anterior parts manifested sensibility when violently pinched: at fifty-two minutes no signs of life remained. Artificial respiration was kept up by intervals till this time. From the quantity of blood lost, and the appearance of the heart, it was pretty certain that death resulted from the hæmorrhage. At sixty minutes, the action of the heart continued, although its cavities were completely emptied of blood, and filled with air.

Experiment 2d.—Took a rabbit four days old, divided the spinal marrow between the occipital bone and atlas with a needle. Respiration was immediately suspended, and the animal became convulsed, struggled and gaped, its strength rapidly diminished, and in three minutes, life was almost extinguished. Artifical inflation of the lungs being now commenced, in two minutes the animal was much revived, and became still more lively as the process went on. At seven minutes from the commencement of the experiment, a wire was passed through the whole of the spinal column, which immediately extinguished the sensibility and motion of the whole body. With a view of ascertaining the condition of the circulation, a hind leg was amputated, which furnished no blood, and, the femoral artery, on being exposed, was found flaccid and empty. Blood was slowly discharged from the veins. The head showed signs of sensibility and life by gaping, as if a respiratory effort.

Experiment 3d.—Took a rabbit of the same age as in the preceding experiment, and under similar circumstances we divided the spinal marrow in the same place, and respiration was immediately suspended. In two and a half minutes the animal had almost expired. Artificial respiration was now commenced, and in half a minute afterwards sensibility returned and the animal revived: we continued the inflation for ix minutes, with the effect of full restoration to the general functions of life. During this period, whenever a cess of the inflation occurred, the animal lost its sensibility a struggled as if for breath.—We now made an incision between the last dorsal and first lumbar vertebræ, and through this opening was passed a large wire, down to the end of the spinal column. The lower extremities were immediately deprived of sensibility and life: the circulation was also suspended in the parts, as amputation of the hind leg produced no hæmorrhage. The inflation of the lungs being continued, two minutes afterwards we passed the wire through the upper portion of the spinal marrow, and sensibility and life, with the circulation, were immediately destroyed in the upper extremities.

Experiment 4th.—The spinal marrow of a kitten was divided as in the preceding experiment, and the same phenomena occurred, namely, instantaneous suspension of respiration, tremors over the whole body, gaping of the mouth, &c. At the end of ten minutes sensibility became almost exhausted, and the inflation was commenced, which being continued four minutes, revived the animal. The carotids were now laid bare, and the circulation was seen to go on rapidly. But on suspending the inflation for a few moments, the action of the heart became much enfeebled, and the blood assumed a dark colour, which soon changed to a florid, on resuming the inflation. This was repeated several times, and always with the same result.

At the end of thirty-eight minutes the carotid and vertebral arteries were tied up, and decapitation performed between the third and fourth cervical vertebræ. A few drops of dark blood issued from the vertebral arteries, which increased in quantity, and changed in colour, upon inflating the lungs. The mouth continuing to gap, at forty-five minutes the thorax was opened, in doing which, such was the acuteness of sensibility and the power of motion in the animal, that its legs were confined to prevent its struggles.

The action of the heart went on violently, and during the suspension of the inflation, its left cavities were filled with dark blood, which soon changed to florid on resuming the inflation. At fifty minutes a leg was amputated which yielded a few drops of blood. The inflation was persevered in for a few minutes, and the animal astonishingly revived, sensibility being manifested on the slightest touch.

At sixty-five minutes we destroyed the cervical parts of the spinal marrow, by passing a wire through the vertebral canal to the first dorsal vertebra. Upon inserting the wire great pain was evinced, but as soon as it passed down all sensibility became extinct in the anterior, while it still continued in the posterior extremities, though it was much diminished. At seventy minutes the wire was pushed through the whole spinal canal, and instant extinction of all the symptoms of life took place. The inflation was resumed for several minutes without any effect. At seventy-seven minutes the heart contained dark blood, and feebly pulsated when we left it.

Experiment 5th.—Laid bare the larynx of a kitten, and separated it from the os hyoides—the cranium was next removed and the cerebrum extracted, which produced no important effect. The cerebellum was then taken out, and the animal still continued to respire perfectly and freely for ten minutes. It stood strong and firmly on its feet, exhibiting symptoms of violent pain, and making several efforts to cry, with partial success. At the expiration of seventeen minutes the medulla oblongata was extracted, which put an immediate stop to respiration. At twenty minutes we commenced the inflation of the lungs, which being continued for eight minutes, the animal was observed to be dead, evidently from the loss of blood.

Experiment 6th.—Took a rabbit four days old, and removed the upper portion of the cranium. The brain was then cut off in slices, and though a considerable quantity of blood was lost, the animal still retained strength to stand and to crawl about. The cerebrum and the cerebellum being removed in the same manner, respiration still continued. In eight minutes, we removed the medulla oblongata, when respiration immediately ceasing, the animal became weak and fell down, showing little sensibility. Three minutes afterwards, we inflated the lungs, and in one minute the animal was much revived. In two minutes we examined the contents of the thorax, and finding the action of the heart to continue, a wire was passed through the spinal marrow, which was followed by an immediate cessation of sensibility and of the circulation. But it is to be observed that the heart continued to dilate and contract feebly.

Experiment 7th.—After dividing the spinal marrow of a kitten, between the last dorsal and first lumbar vertebræ, a probe was introduced, and all the spinal marrow destroyed as far as the first dorsal. The action of the intercostal muscles immediately ceased—a laboured and imperfect respiration was carried on for a few minutes, which was succeeded by gapings. Sensibility appeared completely lost in the body, and in four minutes afterwards much impaired in the anterior, though it was still considerable in the posterior extremities. At five minutes we commenced inflation, and at seven minutes the posterior parts were sensible to the least touch, the gapings still continuing. At ten minutes we destroyed the lumbar portion by running a wire down the canal to the tail. This produced an instant loss of sensibility in the hinder parts, while the anterior continued still sensible. At fifteen minutes the spinal marrow was divided at the first cervical vertebra, which produced no obvious effect. At seventeen minutes, sensibility was still perceptible in the anterior parts, and gaping continued. At eighteen minutes we destroyed the cervical portion, when life immediately ceased.

Experiment 8th.—Dissected down and put ligatures upon the sympathetic and par vagum nerves of a kitten. The animal appeared in the most excruciating agony; and soon ceased to cry and to breathe, though it made strong efforts to do both. At the end of five minutes the ligatures were removed and the larynx exposed: the rima glottidis was rigidly closed, which accounts for the stoppage of the breath. At the end of eleven minutes, when the animal appeared entirely exhausted, a tube was passed into the trachea and inflation commenced. After a few strokes of the piston the animal began a voluntary respiration through the blow-pipe. At twenty-five minutes the spinal marrow was separated at the first cervical vertebra, which put an immediate stop to respiration. At thirty minutes sensibility continued strong, when a probe was passed in at the cervical vertebra, and run through the whole length of the spine, which produced an immediate extinction of life in every part.

Experiment 9th.—Laid open the abdomen of a kitten and tied up the aorta just below the cœliac artery. In ten minutes sensibility was much impaired in the posterior extremities, though by no means extinguished. The posterior extremities were now separated from the body between the first and second lumbar vertebræ, and when thus separated they exhibited sensibility for two minutes. At twenty minutes the animal stood on its fore feet, respiration and circulation being pretty free. The spinal marrow was now divided at the first cervical vertebra, which as usual, produced a suspension of respiration. At twenty-three minutes we commenced inflation, and at twenty-five minutes amputated a leg, which afforded a little blood. At twenty-seven minutes decapitation was performed. Three minutes afterwards we recommenced inflation, and the anterior parts exhibited signs of sensibility when pinched; the head gaped. At thirty-three minutes destroyed all the spinal marrow, which extinguished life entirely. At forty minutes we opened the thorax, and observed the heart pulsating, and on wounding the aorta, dark blood issued.

Experiment 10th.—Opened the abdomen and put ligatures under the aorta and vena cava, leaving them loose. Next we dissected down and tied up the carotid arteries and jugular veins, separated the larynx, and introduced a tube for the purpose of inflating the lungs, through which the animal breathed. In ten minutes we divided the spinal marrow at the first cervical vertebra, which put an immediate stop to respiration. At the end of fourteen minutes we commenced inflation, and at seventeen minutes decapitated, and continued the inflation. At twenty-two minutes the ligatures were tied, which had been previously fixed on the aorta and vena cava, and then we cut off the posterior parts at the last dorsal vertebra. At thirty-nine minutes sensibility was very manifest, the inflation being continued. At forty-two minutes we opened the thorax, and found the heart to beat regularly and strongly. One of the internal mammary arteries having been divided accidentally, bled freely per saltum. At fifty minutes, the circulation going on vigorously, the venæ cavæ were seen carrying dark blood to the heart, and the pulmonary veins returning vermilion coloured. The auricles contracted synchronously, and emptied themselves completely. The ventricles did the same.

At fifty-five minutes, sensibility was very much impaired. One leg, being amputated, did not bleed, although the action of the heart appeared regular and vigorous. At fifty-seven minutes, the aorta being cut, bled freely of vermilion-coloured blood, which caused a gradual diminution in the action of the heart. At sixty-two minutes, however, it had so completely ceased as not to be excited by the point of a needle; though on blowing air into it from the vena cava, its contractions were feebly renewed.

It appears to me, that the foregoing experiments pretty clearly establish the validity of the three positions assumed. That the medulla oblongata is essential to voluntary respiration—that the spinal marrow is immediately connected in its functions with the vitality of all the parts to which it sends nerves, and that the heart is indebted to the spinal marrow for its ability to carry on the circulation. To corroborate these points however more fully, let us review the bearing of the several experiments.

In the first, second, third, and fourth, as the object of inquiry was to ascertain the influence of the medulla oblongata on respiration, a section was made with a needle just below the occipital bone, and a suspension of breathing immediately took place. This proves that the organs of respiration are put in motion by an influence derived somewhere from within the cranium, and that however indispensable the integrity of the phrenic and the intercostal nerves may be to their action, still the primum mobile is not in them or the parts of the spinal marrow to which they belong.

Next it was desirable to ascertain what part of the encephalon maintained the process.

Experiments fifth and sixth prove it to be the upper part of the medulla oblongata, near the origin of the par vagum and glosso-pharyngeal nerve; for in each of them the successive removal by slices of the cerebrum and cerebellum did not arrest respiration. The moment, however, that the medulla oblongata was injured, a stop to this process was the consequence.

We are thus led to a beautiful and important conclusion in physiology, one which, from the unequivocal character of the proofs brought to its support, is justly entitled to the greatest attention, and which ought to enter into all our reasonings upon the symptoms connected with apoplexy and other affections of the brain attended with compression of its substance. It is highly probable, from the cavity of the cranium being completely filled, that any deposits of blood within it by increasing the aggregate mass of contents, or any diminution of its capacity by a part being depressed, which is equivalent thereto, will cause itself to be felt throughout the substance of the brain: the compression being communicated not only to parts immediately contiguous, but also, by a juxta-position of particles, to the whole mass. If this reasoning be correct, we may understand in what manner respiration is affected by compression on the medulla oblongata, notwithstanding the effusion or depression may take place at a very remote situation from it. Conceding this, is it not proper to make perforations in the cranium, to relieve compression, by allowing the brain more space, though we may not be assured that the effused fluid will be exposed?

In relation to this part of our subject is an observation made in experiment eighth.—A ligature placed on the par vagum stopped respiration, by producing a spasm of the rima glottidis; and a blow-pipe introduced into the trachea simply by keeping the passage for the air open, restored the function.

Second, As regards the influence of the spinal marrow upon the life of other parts. In experiment second, a wire passed through the whole of the spinal column, immediately extinguished sensation, motion, and circulation throughout the body. In experiment third, a wire, passed through the lumbar portion of the spinal marrow, destroyed life in the lower extremities. The same injury inflicted on the dorsal portion produced the same effect on the upper extremities. Experiments fourth, sixth, seventh, eighth, and ninth, all demonstrate the same principle, by similar results.

Third, In regard to the manner in which the action of the heart is kept up.* The greater part of the experiments show the circulation to have ceased along with the destruction of the spinal marrow: But the seventh, taken with the other, brings forward an interesting fact, namely, that the action of the heart is not sustained by any particular portion of the medulla spinalis, but by the whole of it, each section contributing its nervous influence.

In one set of experiments we have seen the circulation sustained by the anterior part of the medulla spinalis, the posterior being destroyed, while in another by the posterior part of the medulla spinalis, the anterior being destroyed; and in the seventh, the circulation was kept up by the extreme portions, that is, the cervical and lumbar, the dorsal being demolished.

Connected with the influence exercised in this manner over the heart is a singular circumstance. To keep up a vigorous circulation where one part of the medulla spinalis is destroyed, it appears only necessary to curtail its extent. This may be done either by putting ligatures on the great arteries, or what is still more surprising, by cutting off the head of the animal. To conclude, it will be seen by the result of these experiments, that whether the nervous matter is diffused through the whole system, as believed by Mr. Hunter, or not, the fact is equally well established that the spinal marrow and the medulla oblongata give to it life and activity.

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* By action of the heart is meant that degree of vigour in it necessary to maintain the circulation, and not simply its diastole and systole.

Having performed some experiments with a view to test the accuracy of Dr. Philip's objections to Le Gallois's doctrines, I propose in a future number of this Journal to introduce them; and shall now merely observe, that their results harmonize with what is here advanced.

Art. V. *Thoughts on the Pathology and Treatment of Cynanche Trachealis, or Croup.* By N. Chapman, M. D.

TO this affection, various other names have been applied by the different writers who have treated of it. It is called suffocatio stridula, angina polyposa, asthma infantum, cynanche stridula, angina epidemica, morbus strangulatorius, and in popular language, croup, or hives, the heaving of the lights or lungs, the choak or stuffing, &c. The best nosological title is, *Tracheitis*. It clearly designates the more ordinary nature of the complaint, and at the same time, gives uniformity to our medical nomenclature.

Croup has commonly been considered, as a disease of modern date, and the credit of having originally noticed and described it, is accorded to Professor Home of Edinburgh, whose publication appeared about the middle of last century.* Turning over, however, one of the earliest volumes of the transactions of the Royal Society of London, I find a very distinct account of the disease, illustrated by dissections. The writer, who was an obscure practitioner, describes it as an entirely new complaint, which had suddenly appeared among the children of Cornwall, committing very considerable ravages. It is also said to be particularly noticed by Martin Ghisi, an Italian writer, so early as 1749.*

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* 1765.

Croup is, for the most part, confined to the early period of life, embracing the space between the first and fifth year, and affects chiefly children florid and robust. But I have known it to attack infants within the month, and also adult subjects. [illegible] he illustrious Washington is said to have died of this [illegible] sease. Two ladies of this city, who are now nearly in the m[illegible] dian [illegible] ave attended in repeated attacks of croup. So [illegible], are they predisposed to it, that they sca[illegible] when exposed to the causes. The same li[illegible] be[illegible] nitted to all their children, who are r[illegible] s.

[illegible] some w[illegible] asserted that croup never oc[illegible] hat it is a rare event, cannot [illegible] the fact without explanation [illegible] eat of the disease, undergo [illegible] evinced by the new tone of voice ac[illegible] nables them to resist those causes, that in the previous state of debility and relaxation of the larynx, more especially, were invited to such morbid aggressions. Cases of this kind, however, are still to be considered as rare and anomalous, deviations from the ordinary course and character of the disease.

Notwithstanding what has been so confidently alleged to the contrary, there is not the slightest reason to believe that croup is ever propagated by contagion. It would seem chiefly to arise from the influence of a moist and cold, or austere atmosphere, and hence prevails more generally in the spring than at any other season, and near to the sea or other large collections of water, rather than in inland positions.

By some writers it is affirmed occasionally to occur as an epidemic, and perhaps this may be true. It is certain that the complaint is endemial to particular places, and within very narrow limits. Many situations on the sea-board are so particularly exposed to it, as almost to preclude the raising of children.

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* There is, indeed, some reason to suspect, that several of the much older authorities meant this disease, in the descriptions which they contain of a very fatal species of angina, *without swelling of the throat.*

Croup has been divided into spasmodic and inflammatory, and not a little discussion has taken place on this subject. It would seem to me, that in all cases where it suddenly attacks, it must partake of the nature of spasm. Time is required to induce inflammation, which consists in an altered action of the vessels of a part, affected by comparatively a slow process. No cause, however, more rapidly promotes it than the disturbance occasioned by spasmodic constriction.

The early symptoms correspond with this view of the pathology of croup, and dissections fully confirm it, showing, where death promptly takes place, none of the phenomena of inflammation. But, under other circumstances, where the disease slowly approaches, or is the effect of inflammation of other parts, extending to the trachea, as sometimes happens in measles, scarlet fever, and most of the anginose affections, then it is of a contrary character, and *post mortem* inspections have revealed exactly such appearances as might have been anticipated.

Even, however, admitting the distinction contended for, I am not aware that it leads to any practical difference. Whether *spasmodic* or *inflammatory*, the directly depleting measures will be found equally effectual in the treatment. No remedy is so prompt in the reduction of spasm of high action as venesection, and none so unavailing or inappropriate as the antispasmodic substances. On this point I wish to speak emphatically, since some of the European as well our own writers of high authority, entertaining other notions, have laboured to establish an opposite practice, consisting in the use of musk, assafœtida, and opium, than which nothing can be more false or prejudicial.

Croup variously makes its attacks. It commonly comes on at night, and sometimes without any premonition or exposure to its ordinary causes. The child wakes up with the hoarse, dry, stridulous cough peculiar to the disease, which has been aptly compared to the sharp sound of the barking of a dog, and in other instances to the crowing of the cock. Concomitant with this, there is a distressing difficulty of respira [illegible] menacing, in some instances, suffocation, with a flush [illegible] ce, a quick irritated pulse, an unusual degree of restlessn [illegible] and anxiety, with a sort of indescribable wretchedness. [illegible] child will not remain long in one position, nor can its c [illegible] plaints be in any way appeased. It whines, and cries, and tre [illegible] and s [illegible] to be excessively uneasy, without suffering any [illegible] p [illegible] e pain.

Cases of thi [illegible] re [illegible] bably dependent on spasm, and terminate [illegible] in [illegible] ry short time, where relief is not afforded. [illegible], on [illegible] casions, the disease advances gradually, [illegible] nary catarrhal symptoms, such as heaviness, [illegible] countenance, defluctions from the eyes and n [illegible] r and more shrill cough than usual, and wi [illegible] ees of fever, which, with the cough, is alw [illegible] at night, and especially after the child has [illegible] tely formed, there is no material difference between the two species of croup, and henceforward their progress is nearly, or perhaps exactly similar.

My mode of managing this disease is exceedingly simple, and has hitherto proved so successful, that I always approach it in the early stages, with a greater certainty of curing it than any of the other complaints of infancy or childhood.

Called in the commencement of the attack, I endeavour at once to puke the child very freely, and for this purpose prefer the tartarized antimony, given at short intervals, as being one of the most certain and powerful of the emetics. At the same time I direct the child to be put into a warm bath for ten or fifteen minutes. This is a useful remedy. It rarely fails to promote the operation of the emetic, and will, indeed, alone, sometimes cure the disease. The emetic however, not operating, or if after its operation the desired effect be not realized, I then bleed copiously, and repeat it and the bath. An attack must be extremely obstinate if it do not now yield. Nevertheless, it will occasionally continue with little or no abatement, and under these circumstances, I resort to topical depletion by leeches, or by cups. The cups should be applied to the sides or back of the neck, as, when placed anteriorly, they will by pressure and suction, greatly impede respiration, and sometimes endanger suffocation. Twice I have seen the distress from this mistake so violent, that I believe death would have taken place had not the cups been removed. As means of local bleeding, leeches are very much to be preferred in such cases. Next I put a sinapism or blister over the throat, or in some instances, these may be made to precede the former applications.

The foregoing remedies failing, or where the symptoms become so alarmingly violent as to demand immediate relief, I bleed *ad deliquium animi*. When pushed to this extent, I may almost say, that venesection is invariably successful. As yet, I have never known one instance in which it failed. The moment that syncope takes place, the hoarseness, cough, impeded respiration and fever, disappear.

This valuable suggestion I derived from Dr. Dick, of Alexandria, one of the most original, bold and successful practitioners of our country. It has been claimed I understand elsewhere, with what justice I pretend not to determine. That however the practice was adopted at least thirty years ago, by this distinguished physician, is unquestionable.

To prefer small and repeated bleedings at this period of the disease, as is advised by one of the most authoritative of our own writers, is a pernicious abuse of an important remedy. It may be laid down as a rule, to which there are few exceptions, that in acute diseases, where venesection is at all demanded, it should in the commencement be so copious, as to produce decisive effects. The rationale of the measure seems not to be well understood. Detractions of blood in a small or large quantity operate, as remedial processes, very differently. The former *abates* action only, while the latter *alters* it, or so far reduces it as to enable the natural energies of the system to subvert or overcome it, and to re-establish health. Of this principle, we have illustrations in pleurisy, in fevers, and many other affections, where a single profuse bleeding, timely recurred to, arrests the progress of the case.

Conceding that the loss of blood is necessary to a cure, it will be proper under the circumstances stated, to pursue this course, even wh[illegible] have grounds to apprehend debility. As small [illegible]ire to be often repeated, the aggregate of bl[illegible]s ultimately greater, and more exhausting i[illegible]ts. Besides which, as there is less structural or functional derangement, the convalescence is more rapid and complete. Whether, therefore, with a view to a prompt cure, or to economize the resources of the constitution, or as a security against relapses, or imperfect recoveries, this practice claims a preference.

The disease being broken, which is shown by the removal of the preceding symptoms, and even still more by the restoration of the *natural susceptibility of the system to the action of medicine*, I administer calomel, not in small and repeated doses, as is more generally advised, but in the largest possible dose, in order that it may speedily and most actively purge. In this particular stage of the disease, a thorough opening of the bowels, carries off the lingering symptoms, obviates a relapse, and confirms the convalescence. But should cough or hoarseness, with tightness of the chest, and deficient expectoration remain, I employ the polygala senega as an *expectorant*. It is in extinguishing the remains of croup, that it displays, I think, not the least of its valuable properties. Doubtless, however, it may be used at an earlier period of the disease with advantage as an emetic, though still I prefer the tartarized antimony.

The practice, as here detailed, is applicable chiefly to croup in its forming and early stages. At this period the disease is restricted pretty much to the upper portion of the trachea, and consists either in a spasmodic constriction of the glottis, or inflammation of the membranous lining of the larynx. But, permitted to continue for ten or fifteen hours, and sometimes even in a shorter interval, it extends itself to the bronchiæ, and into the substance of the lungs, producing sooner or later vast collections of mucus and phlegm, or exudations of coagulable lymph, or an engorged state of the pulmonary organs with blood.

The symptoms at this critical conjuncture are materially different. Now we have all the manifestations of an interrupted and defective circulation. The lungs, loaded and oppressed, very imperfectly execute their functions. The complexion is mottled, and the cheeks have a circumscribed flush, with some mixture of lividness. The eyes are prominent and inflamed. The pupil is often widely dilated, attended by an expression of countenance wild, haggard, and ghastly. The respiration is exceedingly laborious, with a full and disturbed pulse, or, the child, sinking under the disease, has its breathing rather more tranquil, with a weak and irregular circulation.

The symptoms in these different states of the lungs are so analogous, that it is not easy to establish, in all instances, satisfactorily, a diagnosis. But though difficult, it is a point of some consequence to be determined, as the treatment in every respect is not precisely the same. To arrive at a just conclusion, we must take into view all the circumstances appertaining to the case in its several stages, as well as the existing appearances.

Of the nature of bronchitis, and especially of that form of it, which resembles catarrhus suffocativus, or in other words, when it proceeds from collections of phlegm, or mucus, or lymph in the bronchiæ or pulmonary cells, I have mostly found that the case has had its origin in catarrh, and which has run a course more than ordinarily protracted. There is also at the time greater or less discharge from the lungs, or at least evidence of heavy accumulations of matter, with an inability to throw it up, and to which may be added, that the pulse is languid, and the surface cold and clammy. But occasioned by sanguineous congestion, however oppressive the dyspnœa may be, there is little or no cough or pituitous discharge, and, what is very distinctive, an entire absence of the *wheezing*, so general a symptom in the first case. The respiration, however, is singularly hurried, panting, and laborious. The pulse too, is full, though irregular and disturbed, and very readily compressible. Cases of this sort, moreover, are apt chiefly to occur in florid and plethoric children, or, as I have seen, in directly the reverse, the weak and valetudinary, and generally this condition is disclosed at an earlier period in the disease.

The indication now, in each shape of croup, is to relieve the lungs of oppression, and to re-establish a free and equable circulation. To effect these purposes, the child should be placed in a warm bath, and while there, copiously vomited by an active and stimulating emetic. The sulphate of zinc has been recommended, and is useful, though the tartarised antimony, with calomel and ipecacuanha, or the juice of garlic or onion, is preferable.*

But in the second case, having pursued the same measures, we are also very cautiously to draw blood—taking away a little at once, suppress the flow, and watch the effect on the system. Being beneficial, we may renew the bleeding from time to time, till our views in this respect are attained.

The necessity of such extreme circumspection in the use of the remedy in this case, is readily explained. Engorgement of the great viscera, and especially the lungs, takes out of the general circulation such a large portion of blood, and confines it so closely in the part, that any considerable loss by venesection is very sensibly felt, creating in some instances, prompt and irreparable exhaustion.

Where the lancet is altogether forbidden, cups or leeches may be substituted, and will be most serviceable on the back.

In each species of the disease, the vesicating applications are highly important remedies. The blister should be put over the breast, or if the case be so urgent as not to admit of delay, some means of more prompt vesication may be resorted to, as cloths wrung out of hot water, or what perhaps is better, pledgets of lint dipped in a decoction of cantharides, made with the spirit of turpentine.

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* These latter are very certain and active emetics, and will frequently succeed in exciting vomiting when the officinal articles have failed.

The subsequent treatment consists principally of the pretty constant use of expectorants, and for this purpose, the antimonial wine, the oxymel or vinegar of squills, the decoction of Seneka snake root, either alone, or in combination with the carbonate of ammonia, will answer exceedingly well. The hive syrup is here a very useful preparation.*

Much may also be expected in some instances from the liberal exhibition of calomel. At all times an exceedingly active expectorant, by which I mean whatever enables the bronchial structure to disengage and expel an oppressive load of matter, it seems, under these circumstances, occasionally to operate with really a specific efficacy. There are some indeed of the respectable practitioners, both of this country and of Europe, who trust almost exclusively to it.

Calomel was originally employed in croup, by the late

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* This is a prescription of Professor Coxe, which I have reason to believe has justly acquired great popularity in the treatment of croup. It is prepared agreeably to the following formula, and the dose is about a tea-spoonful for a child of one or two years old.

Syrupus Scillæ Compositus.

℞ Seneka Snake root, bruised,
Squills, dried and bruised, ā ℔ ss
Water, ℔ 8

Boil together over a slow fire, until the water is half consumed —Strain off the liquor, and add

Strained honey, ℔ 4

Boil them together to six pounds or to the consistence of a syrup—add to every pound of this syrup, sixteen grains of tartar emetic—that is, one grain to the ounce.

See American Dispensatory, 4th Edit. p. 343.

Dr. Kuhn, of this city, who prescribed it so early as the year 1770. The Scotch physicians are devoted to the remedy, and consider it almost infallible, or such rather seems to be the opinion of some of the most distinguished of their writers. By one of them it is said, "that in every case where it was employed, previous to the occurrence of the lividness of the lips, and other mortal symptoms, it has completely succeeded, both in curing the disease and in preventing any shock to the child's constitution." His manner of exhibiting calomel would appear daring, even to rashness, were we not acquainted with the insensibility of the system in this disease to remedial impressions of every description. To a child of two years old, he has given upwards of one hundred grains, in twenty-four hours.

With Dr. Hamilton, to whom I have alluded, the professor of midwifery at Edinburgh, I am acquainted, and from his high standing and character I entertain not the slightest doubt, with some allowance for an undue enthusiasm of expression, of the veracity of these representations. Nevertheless, I will not take upon myself to support or recommend his practice. The mode which I have suggested of managing this disease, at least as it appears in this country, I must think decidedly more effectual, and certainly less hazardous, as well as repugnant to popular prejudices.

In the preceding history, I have delivered, very concisely, some account of the pathology and treatment of croup. It results from what has been said, that I consider it at first, as a spasmodic or inflammatory affection of the larynx, and in its subsequent stages as one or the other of the forms of peripneumonia notha—either a congestion of the lungs with mucus or lymph, or with blood. The former, I believe to be by far the most common occurrence, or usual shape, of the disease.

The practice appropriate to the several circumstances of croup, I have also endeavoured to point out with some degree of precision.

It will be perceived, that in relation to the latter stages of the disease, while I maintain that the lungs are affected differently in some cases, the only distinction in the treatment suggested, is the limitation of bleeding, to the *apoplectic condition of these organs*. Though I hold the other state to be essentially bronchitis, and hence originally of an inflammatory character, still, from the early depletory measures generally pursued, such no longer exists. We have on the contrary at this time, as its product, effusions or exudations, obstructing respiration. Yet wherever there is reason to suppose a remnant of inflammation, topical bleeding, at all events, may be, and ought to be practiced.

What, on the whole, I wish especially to call attention to, is the view which has been presented of the nature of croup at an advanced period. It is interesting, not as mere theory, but as leading to the practical improvement on which I have dwelt. Though not generally entertained or adopted, it is most fully established, as well by the phenomena of the disease, already noticed, as by dissections. To this point, we have to a certain extent, the testimony of Cheyne, who has written with ability on the disease, and the still higher authority of Baillie, not to mention other names of less distinction, all which has been confirmed by dissections, conducted in this country.*

Not a little is said of the existence of a membrane in the larynx, and to which so much is ascribed in occasioning death, that an operation has been proposed and even practised for its removal. That it does occasionally exist cannot be d[illegible]ied, though I suspect rarely, as I never met with it, in m[illegible]repeated examinations for this purpose.

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* Neither of these writers, however, have noticed the *apoplectic* state of the lungs. The venerable Dr. Bard, of New-York, who was among the very first to adopt a correct pathology of croup, says in his Essay on the subject, published in the year 1771, *that he has found the pulmonary organs so dense and solid, from sanguineous congestion, that they exhibited the appearance of the structure of the liver.*

Baillie, in his morbid anatomy, tells us, "that when the inner membrane of the trachea is inflamed, it is sometimes lined with a layer of a yellowish pulpy matter. This does not adhere very firmly to the inner membrane, but may be easily separated. It extends from the upper part of the cavity of the larynx, into the small branches of the trachea, which are distributed through the substance of the lungs. There is at the same time a good deal of mucus in the trachea and its branches, together with a mixture of pus. This is the appearance of the inside of the trachea, in patients who have died from the croup." Cheyne's account of the post mortem appearances in the disease, so closely resembles the preceding, that I consider it unnecessary to recite it.

Vide his Essay on Diseases of Children, Edinburgh, 1801.

Th[illegible]earances I have observed in dissections relating to [illegible]nx were slight marks of inflammation, with more or l[illegible] mucus, such as is formed by all the secreting surfaces. Why I have not seen the membranous production is ne[illegible] e[illegible] explanation. To throw out coag[illegible] is composed, requires the vess[illegible] tate which, by the copious deple[illegible] hat came under my notice, was p[illegible] bl[illegible]

Eve[illegible] red of its existence, I do not know [illegible], the operation would do more [illegible] ase at this time has reached the [illegible] f in this way could be expected. [illegible] appen, though seldom, that it is restrict[illegible] to the [illegible], and that respiration may be so obstructed by the membrane alluded to, or from an accumulation of mucus, as to threaten the immediate extinction of life. By the removal of the mechanical impediment, an operation might be useful, and has actually proved so in two cases recorded in the foreign journals, in which relief was instantly afforded, and ultimately recoveries took place. I have now in my possession a drawing executed by my friend, the late professor Dorsey, representing the membrane in a position completely to intercept the passage of air to the lungs, and which, had it been displaced by an operation, as he strenuously proposed, the child would probably have been preserved.

In the estimate of this resource of our art, we ought, moreover, not to overlook the fact of the striking effect, in many cases, from the expulsion of the membrane by vomiting or coughing, and sometimes in a state of things too, the most critical and alarming. Yet it seems that the operation, on the whole, is deemed a very desperate and precarious expedient, to be held in reserve only for the extremest emergencies, and where common measures have altogether failed.

Two causes have conspired to render croup, which is not necessarily a fatal disease, so much so, that it is placed by some writers even among the *opprobria medicorum*, and by most practitioners is considered a highly obstinate and dangerous affection. The first is an erroneous notion regarding its pathology, and the second the careless and feeble mode in which it is commonly managed.

An impression almost universally prevails, that children, owing to an extreme delicacy and frailty of constitution, cannot bear any very vigorous impression from remedies. As a natural consequence of such an opinion, the general practice in their complaints is extremely inert, exactly indeed of that kind, which has been facetiously described as observing a strict neutrality between the patient and the disease, neither declaring for the one nor the other.

By no narrow or partial observation, I am thoroughly persuaded that the very contrary of this opinion is true. Children, I have remarked, display an uncommon *tenacity of life*, and strength of constitution. They often survive under circumstances which destroy adults. They have been found living at the breasts of their mothers who had perished by exposure to cold, as is recorded by travellers and other writers. They confessedly resist contagion better than grown people, and when attacked, more certainly recover, not only from diseases of this description, but from all others, when properly treated.

Nor is this all. They sustain better the operation of the most active remedies, as vomiting, purging, sweating, blistering, and I add, without hesitation, *bleeding*.

During the growth of the body, the *fluids*, in relation to the *solids*, are larger in proportion, as is distinctly proved. This fulness of their vessels and greater excitability of system, render children peculiarly liable to inflammatory attacks. Nearly all their complaints partake of this character.

It follows therefore, that they require *oftener to be bled*, and my own experience convinces me, that venesection may be resorted to in their cases, with more safety, and decidedly greater advantage. No one who is conversant with their diseases, and has practised venesection much in them, can withhold his assent from the accuracy of this statement.

Endowed with superior *vital energies*, children have, moreover, very extraordinary *recuperative powers*. They notoriously recover more [illegible]ly from wounds, and injuries, and s[illegible]tio[illegible]d recruit with greater rapidity after [illegible]e[illegible]r by disease or by remedies of any description.

It is on this a[illegible]ile there is any indication of life, how[illegible] appearances may be, we ought neve[illegible] a child, in an acute disease, as a [illegible] still retaining some hope, to cont[illegible] restorative principle of the constitu[illegible]se generally pursued, I am persuaded, [illegible] unfrequently be rewarded by such cures, as reflect lustre on the art, and give to our skill a glorious triumph.

To do this, however, in the disease before us, the practice must be prompt and energetic, and our attendance unremitted till relief is afforded. It is a rule with me, never to leave a child, in croup, till the alarming symptoms are over. This great degree of vigilance and attention are necessary from the rapid career of the disease, and not less from the extreme and peculiar uncertainty of the operation of our remedies in it.

As a most formidable enemy, in all its presentations, it should be attacked early, vigorously, and on the very outworks. Delay never fails to invigorate its force, and when permitted to get possession of the citadel, or in other words a firm hold of the system, we shall find it always difficult, and often utterly impracticable to dislodge it.

ART. VI. *An Account of the Yellow or Malignant Fever, which appeared in the City of Philadelphia, in the Summer and Autumn of 1820, with some Observations on that disease. Read before the Academy of Medicine.* By SAMUEL JACKSON, M. D., President of the Board of Health.

THE repeated appearance of yellow or malignant fever in our commercial cities; the fatality that attends its progress; the consternation produced by its presence; the immense pecuniary losses sustained by the derangement of the relations of society, and the interruption of the daily occupations and pursuits of our citizens; the private distress and public embarrassments, consequent upon its prevalence as an epidemic; give to the consideration of this disease a general interest, and render it the most important, that can engage the attention of the American physician.

Faithful, accurate and impartial histories of the most probable and obvious causes and origin of this disease, investigated with an unbiassed disposition, in a spirit of truth, and drawn from observations on the spot, not derived from popular rumour, too often a confused medley of invention and fact, truth and falsehood; and correct statements of the symptoms by which it is characterized, the treatment, and success attending it at each period of its prevalence, elucidating its nature and shedding light on its pathology, are among the most valuable of our medical documents.

Many very excellent works and treatises on yellow fever, have been published in this country and in Europe, embracing much interesting information, and presenting a great collection of facts and observations, which have tended very materially to improve our acquaintance with this disease. Our knowledge on this subject, is, however, far from being complete, or opinion settled and confirmed. A discordancy in the results of experience exists, where authority is equal, and fact oppugns fact, with such equal weight of testimony, as renders a decision extremely embarrassing. On some of the most important points, both theoretical and practical, all that has heretofore been written, has left the question wholly undecided, and there still prevails a wide diversity of sentiment respecting them, not only with the public at large, but with the members of the medical profession.

It is not by specious theories framed in the closet, nor by speculative powers alone, however ingenious and acute, that the doubts that obscure this subject are to be dispelled, or the difficulties that surround it are to be cast down. They are to be determined by evidence alone. It is true, that in this case as in all others, the testimony may be, and in fact is, too often vitiated by the concurrent influence of human passions, pride of opinion, and prejudice; or rendered inaccurate by preconceived notions, precipitate conclusions from inadequate and incorrect data; by hasty, careless, and timid observation. But the errors arising from these sources must and ultimately will be detected and corrected, by the accumulation of unquestioned facts; and sound principles be established and verified by repeated and enlarged experience.

In the unsettled condition, in which the doctrines respecting the causes, nature and treatment of yellow fever confessedly are, it is incumbent on those who may enjoy the means of making accurate observations on that disease, not to neglect the opportunity that is presented to them, but to collect and embody with scrupulous fidelity all the facts and circumstances, that are remarked in connection with it.

It is not in the expectation that I am able to offer any new views which shall definitively determine what is now dubious, or completely enlighten what is acknowledged to be obscure, that I am induced to undertake the present publication. But influenced by the preceding consideration, I wish to place on record, a faithful history of the origin and progress, the symptoms, character and treatment of the yellow or malignant fever, as it appeared in the city of Philadelphia, in the summer and autumn of 1820. A comparison can then be instituted between the disease of that year, and those which existed in former, or may be met with in subsequent periods. And it is only by the results of observations of the same epidemic disease made at different and distant epochs, in varying and diverse situations, that we can acquire a correct knowledge of its causes and nature, so as to be enabled effectually to guard against or save ourselves from its invasion, and to establish a proper and successful mode of treatment.

There is another consideration that has considerable weight in calling for this publication. During the prevalence of the disease, many reports were propagated respecting its introduction into the city, which were entirely destitute of foundation. The editors of certain newspapers, seized with avidity the chance to furnish out their barren columns with a "passing paragraph" of news, and gave them currency. Whether they were publishing truth or falsehood, seemed to be a matter of perfect indifference, not worthy of a moment's inquiry, but was wholly lost in the triumph of presenting to the palled appetite of their readers, some novel scrap not derived from the labour of the shears. From some equally worthy motives, certainly with the same indifference to the truth of their publications, the acts of the Board of Health were arraigned, and its integrity impeached, on assumptions without a shadow of truth. Accusations of a deliberate endeavour to mislead and betray the public, by suppressing information, and by falsifying the daily reports, the base invention of the designing, or the ridiculous conjecture of the credulous, were countenanced and circulated in those journals. The Board of Health, acting under the experience of the limited extent and early subsidence of the disease in former years, especially in 1819, both in this city and in New-York, felt assured, that prudence dictated the propriety of avoiding the creation of an alarm, and occasioning the dispersion of the citizens, without a certainty of the absolute necessity of the measure. Whilst the board were occupied in watching the progress of, and daily combating the disease, and giving regular and daily information with regard to it, their conduct was misrepresented, and designs the most unworthy and criminal were imputed to them.

In the foremost of these traducers and alarmists, was the editor of the New-York Evening Post. At the distance of nearly 100 miles, he pretended to possess more accurate information, than those who were on the spot, and daily engaged in attendance on the sick. With assurance rarely paralleled, he pretended to disabuse the community, voted himself to be the guardian and conservator of the general health, pronounced opinions on the nature and causes of a disease, of which he was totally ignorant, a single case of which he had not seen, and if he had seen, he would have known nothing respecting it, in the manner of a first rate dogmatist.

The resident physician of New-York evinced an alacrity of disposition, and a pruriency of ambition to keep pace with his great newspaper rival and compeer, in spreading and maintaining the unfounded reports and exaggerated alarms, that were widely disseminated through the country, and were solely calculated and intended to inflict an injury on the commercial and trading interests of Philadelphia. The laughable and truly ludicrous termination of Williams's drunken frolic, (a malignant case of yellow fever, imported into New-York from South near Callowhill street, and which was blazoned abroad in the public papers,) cut short the career of the learned doctor in the laudable contest with the editor of the Evening Post, who was left master of the field.

The falsehoods and misrepresentations, that have thus been set afloat, may be quoted in a future period, as authorities, should they not be corrected; and it is therefore proper, for the cause of truth, that the facts precisely as they occurred, should be recorded.

The situation I held in the Board of Health during the past summer, gave me an ample opportunity to witness, and obtain information of the disease, and of its progress, from its commencement to its termination; and presented peculiar facilities to make myself perfectly acquainted with all the facts relating to it. These were not neglected, but whatever seemed entitled to attention, or had a bearing on the subject, was carefully noted at the time. Since that period, when more disengaged, considerable pains have been taken to verify, correct, and substantiate the different observations that were collected, in order to free them from any inaccuracies, misconceptions and errors to which they were liable, which were almost inevitable, from being made whilst in a state of constant and active occupation.

Before proceeding to the history of the disease, I will premise a few observations on the weather and the diseases that were its precursors.

From the earliest cultivation of medical science, certain states or conditions of the atmosphere, have been recognized, as powerfully influencing the production of the causes of diseases. Various and widely different conjectures have been made, at different times, of the nature or cause of this state of the atmosphere. Some, as Hippocrates and Galen, have attributed diseases to the natural and known causes of changes in the air, though Hippocrates also speaks of some unknown or divine principle, "το θειον," to the operation of which, he supposes pestilential diseases may be owing. The "*seminarium e cælo dimissum*" of Diemerbroeck, is a similar supposition, and appears to be taken from the "το θειον" of Hippocrates. There are some writers, again, who attribute this state of the atmosphere to some electric operation; while others, especially Van Swieten and Sydenham, ascribe the epidemic constitution of the air to some hidden or occult qualities, derived from exhalations from the bowels of the earth.

The difficulty of deciding on this intricate and obscure point must be acknowledged, and any probable solution of it may well be despaired of at present. The fact, however, that there are certain states or conditions of the atmosphere which may well be termed "constitutions of the air," that more particularly favour the prevalence, not only of certain types of diseases, but certain diseases themselves, cannot be denied, is established by a long series of observations of the highest authority, and is clearly manifested in the history of the diseases of our own country.

Prior to the year 1793 the general type of diseases, as far as we have information, was of the ataxic character; and yellow fever had not appeared as an epidemic since 1762. During the whole of the war of the revolution, when the military operations in the West Indies were of a magnitude equal to any that have since been conducted in that quarter; when large bodies of troops were frequently accumulated in the Antilles, landed in our country direct from them, and every circumstance seemed combined that could generate and propagate the disease, both there and here; still, according to Dr. Rush and other high authorities, yellow fever was a disease entirely unknown to our physicians.

In that year, however, it suddenly burst upon our country with a fierce and desolating fury, with scarce a single premonitory sign to warn of its approach. From that period down to 1805, when it last threatened to assume an epidemic character, it annually prevailed to a greater or less extent in the summer and autumnal months. Other diseases at the same time became more inflammatory and less easily managed; and the typhoid character less prevalent. From 1805 yellow fever was met with in a few sporadic cases only, which seldom attracted much notice, but still served to keep it in remembrance with the practitioner. With the decline of this form of disease, typhus became a more common occurrence in practice, while dysenteries and choleras became less frequent and less malignant. In 1808 began to the eastward that malignant and fatal form of disease, which has been denominated in various places by the names spotted, typhus peteehialis, &c. It reached our city in the autumn of 1812, over which it spread a feeling of gloom and dismay, and will long be remembered as having snatched from their useful and honourable careers some of our most respectable citizens. Among the victims it ushered to the grave, was the eloquent teacher and illustrator of our science, the illustrious Benjamin Rush.

From the time that this disease prevailed, yellow fever became a stranger to us, even in a sporadic shape, and as it disappeared, a kind of interregnum ensued, in which no disease showed itself with violence or to great extent. A period of unexampled health existed, especially during the cool summers of 1814 and 1815. In the summer of 1818 diseases again began to assume a more exalted character; and two well marked cases of yellow fever occurred. In 1819 the summer diseases exhibited a still nearer approach to their former state, and were of more frequent occurrence. On the 23d of June, yellow fever made its appearance in a decided and alarming manner at Reeve's or Market-street upper ferry, where eight cases occurred between that date and the 6th of July. It then subsided in that quarter, and re-appeared on the 29th August, near Swanson-street and Huddle's alley, where fifteen cases occurred up to the 4th October, and one case was met with in Front-street above Walnut on the 23d September, making twenty-four cases of yellow fever in that year, of which number twenty died, and four only recovered.

In the past summer and autumn, diseases assumed the general symptoms which they possessed in the former epidemic periods of 1793, 1797, and 1798. Cholera morbus and infantum were very prevalent; bilious and remittent fevers, from which our city had been for several years nearly exempted, were common diseases; and dysentery, which had become a rare disease in Philadelphia, was of frequent occurrence, and very difficult to manage. From every part of the country accounts received by letters and the papers, represented the season as unusually sickly, and diseases as uncommonly mortal. Dysentery in many places was extremely fatal, and bilious and remittent fevers, along the water courses, assumed a character of peculiar and most unusual malignancy, and differed very little in their general symptoms from the yellow fever of our cities.

This slight and very general outline of the medical history of this portion of our country, which I believe will be found applicable to a large part of the northern and western sections, coincides with anterior observations in Europe to the same purpose, and will demonstrate very clearly that there is some particular constitution of the air, that favours more especially the production and prevalence of certain diseases; stamps prevailing diseases with a particular type, and renders them of a more wide and general diffusion. Whether the prevalence of certain winds, humidity, and heat, are capable of producing, and do produce, this epidemic constitution, correct meteorological observations may hereafter decide. Their importance in enabling us to appreciate all the causes of diseases cannot be denied, and it may be affirmed, without fear of contradiction, that any one who neglects to avail himself of their aid, can be but little relied on, when investigating the cause and nature of this and various other diseases.

The winter of 1819 and 1820 set in severe towards the last of December, until which time it had been open and mild. From December until March there were but few intermissions of cold weather, which was rather more severe than usual. During most of that time, the surface of the earth was covered with snow. Lightning and thunder occurred on the 16th February. The northern lights were witnessed repeatedly in the course of the winter.

Whooping cough had been prevalent during the preceding autumn, but had given way, and meazles were the most common disease. The type was considerably inflammatory, and required active depletion.

The spring was very wet and backward. In March considerable quantities of rain fell. May was also a humid month. There was rain either continued or in showers on eighteen days of that month. 5.04 inches of rain fell. The mean temperature was 66.86.

On the approach of spring the measles disappeared, and scarlatina made its appearance, which continued to prevail until August. A few cases were of a very malignant character.

In June warm weather commenced. The coldness and moisture of the spring months, had checked the early evolution of the vegetable kingdom, which now progressed with astonishing rapidity. The crops of hay and the harvest were more luxuriant and abundant than had been known for many years, and this month being dry and warm, were well got in. There fell 1.20 inches of rain, and the mean temperature was 78.06 degrees.

July was a humid and warm month. The thermometer in a room with a draft through it, and free from reflected heat, stood at eighty-eight and ninety degrees on the fifth and sixth, and at eighty-nine on the thirteenth. The mean temperature of the month was 82.01 degrees. It rained on ten days, from the fourteenth to the thirty-first. The quantity of rain that fell was 4.92 inches.

August was warm but dry. It rained on seven days, but the rain guage marked only 1.98 inches. The mean temperature was 79.08 degrees.

September was also warm and dry. There was rain on three days only, and the quantity that fell was only 1.56 inches, of which 1.16 inches fell on the twelfth. The mean temperature was seventy-five degrees. There was frost on the twenty-fifth.

October was remarkable for the quantity of rain that fell. There was rain on nine days. Four inches fell on the third, and 3.60 from the fifteenth to the sixteenth. The whole quantity of rain was 11.37 inches. The mean temperature was 58.08 degrees.

In the month of May, a fever of a bilious and remittent character, combined with typhoid symptoms, appeared among the blacks. It continued to spread during the months of June and July; in the latter part of which month, it obtained its height, and was seen in its most aggravated forms. It declined through the month of August, and terminated as an epidemic in September. Between four and five hundred persons were affected with it. It attacked occasionally a few whites of the poorer class, but not more than about twenty or thirty on the whole were attacked with it. It was so generally confined to the blacks, that it acquired the name of the negro fever.

It was preceded by a chill, and was accompanied with pains of the head, hot skin, tenderness of the epigastric region, irritability of the stomach, and bilious vomitings. The tongue was moist and white at the beginning, but became dark, foul and dry in a few days. The adnata was of a dusky hue, occasionally yellow, and often blood shot. The patients were generally sleepless, and lay with their eyes wide open. The pulse was seldom tense, but soft, yielding and frequent. In the last stages there was low delirium, great insensibility, tremors of the tongue, which was black, or loaded with foul sordes, and a general collapse of the system.

Lenient purgatives, demulcent and sub-acid drinks, and mercurial preparations, combined with mild diaphoretics in the first stages; blisters, sudorifics and gentle stimulants, with nutricious diet in the second; wine, bark, serpentaria, milk punch, toddy, and the like, in the last stage, constituted the general treatment.

The disease was quite manageable, when placed under treatment in its commencement, and proper care could be taken of the patient. But most of those, who were its subjects, were in so wretched and miserable a condition, living in confined and crowded rooms, amidst every kind of filth and vile garbage; were persons generally of vagabond habits and lives, and were constantly surrounded by the debauched, vitious, and intemperate, that it was impossible to afford them proper medical aid, unless removed to the public institutions. Few indeed could be induced to persevere in attendance, amidst scenes of dissoluteness and misery, the senses constantly offended with the most nauseous exhalations and disgusting exhibitions, and finding prescriptions and advice almost wholly unattended to. I have often met with instances, where it was difficult, sometimes impossible from a want of sympathizing feeling, even amongst the friends and relatives of the sick, to induce them to go a few squares to obtain proper remedies. In the Alms House establishment the deaths were one in six.

The chief theatres of this disease, were the lanes, alleys and courts, inhabited by the negroes in the southern and western parts of the city. It was also found, though not so extensively, in the northern section. These places being principally of a less width, than is required by law to constitute them public lanes and alleys, they are never cleansed by the public scavengers, and were in the spring in a most abominably filthy condition. Few of them are paved, and the offals and accumulated filth of many years, are collected in them. The wetness of the spring had kept this heterogeneous mixture of fermentable and putrifying matters, in a soft, lutulent state, which was soon set in action by the increasing temperature of the advancing season. The owners of property, who have thus cut up and divided their lots in the city, in the manner that has, within a few years, been done, by which they have formed as it were a number of alembics, distilling poisonous exhalations, deserve the most serious reprehension, and have created a great and alarming evil, which ought to be early corrected by the public authorities. In the latter part of May, several cases of this disease appeared in a court opposite the Alms House, where there existed a considerable extent of a half fluid mud, in which were decaying the heads and entrails of shad and other kitchen offals, and from which bubbles of gas were seen constantly disengaging. The city commissioners being informed of the circumstance, immediately ordered it to be removed. Mr. Stiles informed me, that of seven men who were put to work on it early in the morning, three were obliged to break off at nine, affected with vomiting and sickness; and he himself was affected with nausea and vomiting, brought on while giving directions and superintending the work.

The course of this disease was watched with great solicitude by the Board of Health, who, as far as their powers permitted them, endeavoured to remove its obvious causes. Its occurrence at so early a period of the year, and the peculiar features by which it was characterized, were no favourable prognostics regarding the health of the city in the approaching warm months.

While in this state of anxious feeling, the attention of the Board was directed by Dr. R. Harlan, on Monday the twenty-fourth of July, to John Hays, living at No. 168 North Water Street, who was ill of a disease of a suspicious nature. I visited him the same afternoon in company with Dr. Knight the port physician. He was found in a dying state. He had served as a sailor on board the ship General Wade Hampton, and had arrived about two weeks previous to his illness from Charleston, South Carolina. From the period of his arrival, he had been daily on the wharves looking for employment. His skin and eyes were of a yellow hue, and the remains of what he had vomited in a basin, the chief part of the contents of which had been thrown out, were of a deep brown colour. He died the same night.

Immediately after visiting Hays, Dr. Knight mentioned, that he had been called the day previous to attend J. Jackson, a young man residing at Mrs. Williamson's, in Water Street, a short distance below Race Street, of whose disease he entertained a strong suspicion, and intended to report the case to the Board the next day. He (Jackson) was immediately visited, and his symptoms were found to have assumed so decided a complexion, as to leave no doubt with respect to the nature of his disease. He died the next night (Tuesday) in convulsions, and with black vomit.

Dr. Knight reported, Wednesday 26th, two women ill with fever of a suspicious aspect, on Race Street wharf. Being in a destitute condition, and having no means to command proper attention, they were immediately removed to the Lazaretto. One died the next day, the fourth of the disease, with black vomit; and the other two days after removal, the fifth of her disease, with the same fatal symptom.

The same day Mrs. Philly, living in a range of buildings situated on Hodge's wharf, and adjoining the house in which Jackson had died, was taken sick. The disease did not manifest its character decidedly until Friday evening. She died next morning (Saturday) with black vomit. In the room below that occupied by Mrs. Philly, lived Mrs. Double, who was advanced to the eighth month of her pregnancy. She was attacked by a smart fever on Thursday the 27th, the symptoms of which indicated malignancy. Her eyes were greatly inflamed, and had a fiery expression. She was delivered on Friday night. After a temporary calm on Saturday, the symptoms became more aggravated, and she expired on Sunday morning.

It was ascertained at this time that a woman of the name of M'Laughlin, whose husband kept a grog-shop at the end of the range of buildings on Hodge's wharf, had died about the 17th of July, after an illness of two or three days. No medical advice had been called in; but from the history of the case, collected from those who had seen her, it is highly probable her disease was malignant fever, and the first case that had occurred, although at the time it was supposed to be cholera morbus.

On Saturday the 29th, Dr. Knight reported the cases of Mrs. Sturgis and daughter, living in Front Street, the second house north of Race Street. Mrs. Sturgis died on Tuesday August 1st; the daughter recovered.

The same day (Saturday 29th) Mr. Le Compte, who kept the New-Orleans Hotel, in Water-street, and whose back buildings open upon Hodge's wharf, was affected with a severe chill, which was succeeded by fever. He had slept the night previous on a table before the back window, with a current of air blowing over him. He conceived his complaint to be merely a cold, resulting from that imprudent exposure, and could with difficulty be persuaded to take advice. He was removed on Monday the 31st to the country, and died on the following Wednesday.

An apprentice lad of George Scott, cooper, in Front Street, a short distance north of Race Street, was reported by Dr. Knight, on Tuesday August 1st, as labouring under fever with suspicious symptoms. He had been working on the wharf near Race Street. He was immediately removed to a temporary hospital recently opened, where the disease, yielding to active purging, assumed a remittent type, from which he rapidly recovered.

Two cases of malignant fever were reported by Dr. Knight on Wednesday, August 2d, as existing in the persons of the daughter and servant girl of Mr. Charles Hill, in Water Street, the second house south of Le Compte's. They were removed the next day; the servant girl to the temporary hospital, and the daughter into Jersey. The servant girl died on Su[illegible]h, with symptoms of great malignancy. The daughter recovered.

Saturday, August 5th, the daughter of —— Thompson, living in Water Street above Race, was reported to the Board. The symptoms were of a suspicious aspect. She was removed on Sunday morning to the temporary hospital; active purgatives and a small bleeding were employed, under which treatment her symptoms soon subsided into those of remittent fever, and she was dismissed well on the 9th, when the temporary hospital was closed.

The foregoing comprise all the cases of the disease that occurred in the vicinity of Race and Water Streets. It will be seen that *twelve* of them were decided cases of malignant fever, of which *nine* proved fatal, and *two* recovered; while there were two persons affected with symptoms of doubtful character, who both recovered.

The removal of such of the sick who had not the means to procure proper attention, and whose situation would permit it, was determined on from the recurrence of several cases seeming to mark this vicinity as infected; and the immediate removal of the inhabitants was deemed a necessary measure. As there was a number of families crowded into the houses that were the theatre of the disease, whose circumstances would not enable them to procure a place of refuge, the west wing of the City Hospital was appropriated for their accommodation; and a small building near the Schuylkill, the eastern wing of the City Hospital being occupied by the society for the relief of children affected with the summer complaint, was taken and opened as a temporary hospital for the reception of the sick. By these means, in a few days, most of the houses exposed to the infection were emptied of their inhabitants. When that measure was accomplished, fences were erected (on Monday 31st July) cutting off the approaches to Hodge's wharf and dock, which appeared to be the focus of the disease.

The two cases of malignant fever in the family of Mr. Hill, on the 2d of August, were the last that occurred in the vicinity of Hodge's wharf. The removal of the inhabitants from the neighbourhood, the erection of fences preventing approach to the source of infection, and clearing away the offensive matters, as far as was practicable, appear to have completely extinguished the disease, which had appeared there with a threatening and alarming aspect.

For some days a perfect calm ensued; and a hope was indulged, that the anxieties and apprehensions which had been entertained for the health of the city, would soon be dissipated, in a general confidence of an exemption from the calamities, attending the threatened presence of the formidable visiter. But these pleasing anticipations were soon disturbed. On the 9th of August, Drs. Hartshorne and Moore invited me to visit with them Mr. Jesse Smith, who had been taken ill on Sunday the 6th, and who had strong symptoms of malignant fever. He died the next day. The clerk of Mr. Smith, Mr. Annesly, was known to be ill at the same time. He had also been attacked on Sunday with great violence, but was recovering. The counting house of Mr. Smith was on the wharf above Walnut Street. These gentlemen having been engaged on Hodge's wharf, a short time previous to their illness, in removing some sugar which had been landed there, it was supposed they had contracted their disease at that place; but as numerous cases immediately succeeded theirs, all originating in the vicinity of Walnut Street wharf, it is more probable they derived the infection from the common cause existing in this new seat of the disease, and that they were the first on whom it displayed its malignant influence. The same day that Mr. Smith's illness was made known, it was ascertained that a man of the name of Forsyth, was ill with a suspicious disease in Water near Walnut Street. He had been attacked on Sunday, and died on Thursday the 10th. His case could not be traced beyond his immediate neighbourhood. In these three cases the lancet had been freely used.

On Thursday the 10th, Dr. Wood reported Mr. Ezekiel Edwards, a clerk in the counting house of Messrs. T. P. Cope & Son, on Walnut Street wharf. He had been taken two days previous, and the disease then displayed the most marked symptoms of malignancy. He expired on Friday, the fourth day of his attack. After strict inquiry it was found, that Mr. Edwards could not have contracted the disease at any other place, than the one where it was first developed, and a suspicion began to be entertained, that there was some mischief lurking about Walnut street wharf.

Dr. Elijah Griffith reported on Saturday the 12th, —— M'Leod, who attended on the store of his brother George M'Leod, on the wharf above Walnut Street, as ill with malignant fever; and on the same day Dr. S. P. Griffitts mentioned the son of Daniel King, who attended a store on the wharf below Walnut Street, whom he considered as attacked with the same disease.

I visited Mrs. Duffy in Walnut Street below Water, on Sunday the 13th. She had eaten heartily of lobsters on Saturday evening, and was taken in the night with vomiting and considerable fever. She complained of violent headache, pain in the back and limbs, with great sensibility of the epigastrium. The skin was hot, face flushed, eyes much inflamed, and tongue furred.

The night before Mrs. Duffy's attack was known, Mr. Abraham Barker was taken very ill. So many cases of disease occurring in so short a period, all of which appeared to draw their origin from the neighbourhood of Walnut Street wharf, rendered it no longer to be doubted but that the disease had broken out in this situation.

On Monday morning the 14th, it was ascertained, that there were several other persons ill in the same neighbourhood, but as most of those who were then confined with the disease, were in the first and second day only, and the symptoms of several of them were but slight, it was thought proper to consult with some of the principal of our physicians on the subject. They were accordingly hastily invited to assemble at the Health Office at three o'clock. By that time, the number of cases known as existing or having occurred near to Walnut Street wharf, from the 9th to the 14th, amounted to seventeen. The result of the conference with the gentlemen of the faculty, was a unanimous recommendation immediately to remove the inhabitants residing in the district then supposed to be infected, and to prevent all intercourse with it, by the erection of barricades. This plan was carried into execution without delay, and by Tuesday evening the greater part of the people were removed, and the fences were erected. New cases were, however, almost hourly reported, and by Tuesday evening, the 15th, amounted to twenty-six. From this period to the 26th, sixteen cases occurred. On the 26th the City Hospital was opened under the care of Drs. Hewson and Chapman, who had in a most handsome manner, and with a generous and disinterested spirit, tendered their services to the Board of Health; and the sick in the temporary hospital were transferred to it. The disease at this time appeared to be in some degree arrested in its progress, and was confined to a very limited extent; for every case was satisfactorily traced to a space bounded on the north by Tun Alley, on the south by Ross's wharf, and by the east line of Water Street.

From the 16th to the 22d, but one case occurred. On this last date, Miss Anderson, living in Water between Market and Arch streets, became a subject of this fatal malady, of which she died on the third day of her illness. A young man who was clerk to Mr. Anderson, left the city the day that Miss A. was attacked; but he was taken ill with the disease a day or two after his departure, and died of it. Neither of these persons had any communication with the infected districts, but appear to have received the disease in their place of residence. When I come to point out the different supposed causes of the disease, I will treat on these cases again.

For : w days no additional cases were reported. On the r. Wood requested me to visit with him Mr. 's family, in Front Street between Walnut and Chesnut Streets, in which he and Dr. Parrish were attending the two sons and a black servant. The elder son was in the third, and the younger in the first day of the disease. The symptoms were unequivocal. The next day (Monday) a daughter, who had been in the country a few days, returned, and complained of being ill. Her symptoms rapidly unfolded themselves. The two sons died on Wednesday, and the daughter on Thursday, all with the black vomit. The servant, who was sent to the hospital, recovered rapidly.

Adjoining to Wharton's lived a person of the name of Drinkwater and his sister. The sister was taken sick on the 28th, and died on Friday the 31st of August. Drinkwater complained of being unwell on Wednesday the 29th, but continued about. He was violently attacked in the evening, became immediately comatose, and died the same night in convulsions. This was one of the most violent and rapid cases of the disease that occurred. Next to Drinkwater's, a lad, apprenticed to M'Intire, was also ill with the disease at the same time. He recovered. On the appearance of the disease in Front Street, it was enclosed with barricades, and the inhabitants from Chesnut to Dock Streets removed.

On the 29th of August, a boy of the name of Lodor, living in Duke Street, in the Northern Liberties, between Front and Second Streets, became affected with the disease, and died on the 2d of September. An examination *post mortem* left no doubt as to the nature of his complaint. It was reported that he had taken the disease by entering the infected district; and it was also reported, that he and his father, who was seized with an apoplectic fit the day the son died, had been engaged in pilfering the stores on the wharves below Chesnut Street. This story was wholly devoid of foundation. In the investigation of the origin of the disease, which prevailed at a subsequent period in Duke Street, the history of this case will be stated, and the probable source of the disease in that quarter will be pointed out.

From this time, the 30th of August, to the 6th of September, there were about twelve cases, which were either directly ascertained to have had communication with the infected district near Walnut Street, or in which circumstances rendered it a probable occurrence.

On the 6th of September, two cases were reported in Lætitia Court, in the persons of two young women, one of whom died the same day, in neither of whom could the disease be traced beyond their residence. A man who worked in a hatter's shop, a short distance below Lætitia Court, in Market Street, the back buildings of which nearly adjoined the house in the court in which the two girls were ill, was also taken down on the 6th; and on the 7th a lad of Mr. Sexton's, two houses above the last case, was also attacked.

From an attentive examination, there is no cause to doubt, but that these four cases originated in that place, there being no evidence of either of them having approached the barricades. In a letter I received from Dr. Fithian of Woodbury, he mentions that he attended a young woman in Jersey, who died with all the symptoms of malignant fever, and who was said to have breakfasted at the tavern in Lætitia Court, at which the two girls first affected lived.

A woman of the name of Cail, was reported on the 6th of September, by Dr. H. Klapp, ill with malignant fever, in Second Street near Shippen. Her disease took place on the 3d. She had been at Arch Street wharf a few days previous to her illness, where she had remained but a short time; in going and coming she had passed through Second Street, and consequently had not been in the sphere of the infection in its original seat, at Walnut Street wharf. She died on the 7th. On the 5th, Deborah M'Devitt, living next door to Mary Cail, was taken with the same disease; she died on the 10th. A Mr. Gibson had been attacked on the 2d or 3d, in Shippen Street, and about the same time several of the members of Mr. Gaw's family, in the same neighbourhood, were taken ill, but whose disease Dr. Klapp assures me was bilious fever. On the 16th, Mr. Joseph Hartley, next house to M'Devitt's, was reported a case of yellow fever. In the family of Mr. Rees, the house next south of M'Devitt's, two boys were ill, within a few days of Mr. Hartley's attack, but were considered cases of remittent fever. On the 2d of October, a recruiting sergeant was attacked by the disease, in Shippen Street, a few doors below Second Street, and died on the 5th,—making five decided cases in this neighbourhood, and four others of bilious and remittent fever.

As the disease was declining in this situation, and in the centre of the city, it suddenly broke out in the Northern Liberties, in Duke Street, two miles distant from Shippen Street, and one mile from the other districts where the disease had prevailed, where two cases occurred on Monday the 18th. There had been a case a few days previous in the neighbourhood, at the corner of Front and Noble Streets. Six more persons were attacked on Wednesday the 20th, all of them in the evening, and two on Thursday the 22d. The disease proved fatal in all these instances. A woman who lived in Second Street below Vine, and who contracted the disease whilst nursing her sister, ill at the corner of Front and Noble Streets, was the only one that recovered. A man in Green Street was attacked about the 8th of October, and died on the 14th, making twelve cases in that *location* of the disease, of which one only recovered.

After the disappearance of the disease in the vicinity of Duke Street, eight cases occurred in different parts of the city the last of November, none of which could be traced to any particular spot. The heavy rains which fell at that time, and the cool temperature of the air, probably terminated the disease, which threatened for many weeks to pour on the city all the afflictions of a mortal epidemic.

The preceding narrative contains a general history of the cases of the disease, from its commencement to its termination. It will be seen, that the whole number was one hundred and twenty-three, of which eighty-one died and forty-two recovered. The following table, will exhibit at one view, the number of cases in each locality of the disease, and the relative mortality in each.

SITUATION.	Cases.	Died.	Recovered.
Hodge's wharf and vicinity, - - - - -	13	9	4
Walnut Street wharves, and Water Street east side from Tun Alley to Ross's wharf, about 700 feet, - - - - - - - - -	47	25	22
In Walnut Street and west of Water, - -	8	5	3
Front Street between Walnut and Chesnut, and Norris's Alley, - - - - - - - -	11	9	2
Front Street below Walnut Street, - - -	4	1	3
Water Street between Arch and Market Streets (Anderson's) - - - - - - -	2	2	
Letitia Court and Market Street, - - -	4	1	3
Second Street near Shippen, and Shippen, -	5	3	2
Duke Street and vicinity, - - - - - - -	12	11	1
Scattered in various places, and which could not be satisfactorily traced, - - - -	19	17	2
Total,	125	83	42

From this statement it is obvious how unfounded were the stories of the suppression of information of the disease by the Board of Health, the invention of the designing, or the weak conjectures of the credulous, which were industriously propagated by those inimical to the prosperity of our city. The misrepresentations that were published in the New York Evening Post, in the form of extracts of letters from Philadelphia, depicting the ravages of the disease in our "ill fated city," in gloomy and frightful colours, are now exposed. Whether those fabrications were the production of Mr. Coleman's fertile genius, or the labours of his correspondents, rests with him to settle; but of their character no one can entertain a doubt.

The conduct of the Board of Health of New York, in the hasty adoption of a non-intercourse between the two cities, and the unusual, unnecessary and unjustifiable severity of the restriction imposed, cannot be passed by unnoticed. I do not mean to condemn the enforcement of precautionary measures. I believe them to be essential to the preservation of the health of large and populous cities. But as in their adoption we violate the sacred duties of hospitality, as well as the charities of human nature; as they compel us to infringe the most divine maxims of our religion, by adding affliction to suffering; they should be the result of a cool and deliberate conviction, that the danger is immediate and pressing; and be imperiously demanded by the necessities of self preservation. When the social relations between the members of our republic, are violently deranged on slight and frivolous grounds; when, by the interdiction of communication between one city and another, established in a careless and hasty manner, without any serious necessity, one becomes an injuring party and the other an aggrieved party; feelings of enmity and hostility will arise; retaliation will be felt a necessary measure, and will be adopted; and a war of petty injuries be commenced, under a pretext of safety, whenever a slight occasion arises for its exercise. This is a state of things highly to be deprecated, and most cautiously guarded against.

The frank manner in which our Board of Health communicated to the Board of Health of New York, not only every case of malignant fever, but every case of suspicious fever; and the circumstances attending the prevalence of the malignant fever in New York the preceding summer, ought to have inspired a perfect confidence in our Board and its measures, and have led to a very different course than was pursued by the New York Board of Health.

The facts presented to them furnished the most ample proof, that the disease in Philadelphia was similar to that in New York in 1819; and clearly exhibited their coincidence in a probable local origin, a certainly local situation, and like that disease, was not communicated by the sick to those in health when removed from the infected district, or if possessed of that character, it was evidently in a very slight degree. At the same time, they were assured, that the measures which they had found so perfectly effectual in arresting and suppressing the disease in the previous year, had been adopted by us, had been perfectly successful at Hodge's wharf, and would be persevered in.

When the city of New York was suffering from the apprehensions of pestilence in 1819, the Board of Health of that city, appeared to repose in perfect confidence upon the security afforded by the removal of the inhabitants from the seat of the disease, and exclusion from it. They did not warn their fellow citizens to desert the city, but endeavoured to lull their apprehensions, by assertions of their confidence in the means they were pursuing. The result proved the wisdom of their conduct. While a small portion of the city only was subjected to depopulation and restraint, the remainder was preserved in its usual healthy and active condition.

The Board of Health of this city at that period, although the same free communication of the state of their city was not made to them by the Board of New York, as that Board received from ours in the year 1820, relied on the integrity of the official reports, as to the extent and nature of the disease, and contented themselves with watching its progress, and the result of the measures taken for its suppression. They delayed imposing a restriction on the intercourse, until the failure of the experiment and the spreading of the disease, should have rendered its necessity absolutely requisite; and the event bore testimony to the correctness of the system.

Notwithstanding the confidence the New York Board had professed in their system for suppressing the disease in the summer of 1819; notwithstanding the experience they had of its salutary powers, in that year, they appear to have distrusted it in other hands, and in another place. There is an inconsistency in such conduct, that cannot easily be explained. If in 1819, the city of New York could be guarded securely against the ravages of malignant fever, which had broke out in its very bosom, by removing the inhabitants from the circumscribed spot infected, and enclosing it with fences, surely there could be no danger apprehended from the same disease nearly a hundred miles distant, confined to an equally limited space, and that space cleared of its inhabitants, and access to it prevented by barricades and guards. I know not by what process of reasoning the conclusion was drawn, that the same means under the same circumstances, pursued with equal vigour, and more promptly and effectually enforced, should be less relied on, or command less confidence in Philadelphia, than in New-York. As similar effects are produced by similar causes, it must have been the inevitable inference, that the means which had proved successful in New-York, at the Old Slip, in 1819, and at Hodge's dock in Philadelphia, would also be successful at Walnut Street wharf. It became, therefore, the part of citizens acting in a responsible station, who ought to be governed by a deliberative prudence and a reflective discrimination, and not from the impulses of rashness, to have satisfactorily ascertained, by adequate proofs, that the disease could not be controlled and suppressed, before they entered on the strong and revolting measures that were adopted.

That the New-York Board did act without reflection appears evident from their own conduct. They retraced their steps, to a certain extent, in a very few days, by rescinding that part of their resolution interdicting the intercourse, which most absurdly and ridiculously made it retro-active thirty days, although the circumstances of this city remained the same. By this retro-active provision, the non-intercourse was carried back to a time prior to the occurrence of a case of malignant fever in the city, and was rigorously exercised towards the count Survillier, who had been but a few hours at his residence in the western end of Market Street, a mile and a half from the local seat of the disease, about twenty days before he attempted to enter New-York. But in a short period after its enactment, the non-intercourse was so entirely neglected by th board, (it is a fair presumption they were satisfied of it inutility,) that it became a mere form, wholly inoperative. Numbers of persons, by crossing first to Camden, went from this city to New-York, where the examination was so slight, being confined to a mere question whether they had come from Philadelphia, that it was, in almost every instance, easily evaded; and very frequently there was no examination whatever.

The restriction, therefore, enforced by the New-York Board had no other effect, than to make an unfavourable impression against this city in the country, and to prevent the merchants and traders of the South and West from resorting to Philadelphia, under a belief that they could not afterwards proceed to New-York.

But the late development by the Medical Society of New-York places the conduct of the Board of Health in a more serious point of view. While they were pretending to a most lively sensibility with respect to the disease in Philadelphia, and spreading in every direction an unfavourable character of our city, the disease, it appears, existed in the very heart of their city, to a greater extent, and with more alarming appearances, than it did with us. The whole number of cases of yellow fever in this city, was one hundred and twenty-three, of which eighty-one proved mortal. The deaths from that disease in New-York, in Bancker Street alone, were one hundred and fifty. Yet they were perfectly silent respecting it. They brow-beat the physicians who reported to them; they refused to publish the reports, and endeavoured to suppress a knowledge of its nature and character, by designating it as typhus.

At the very time, then, that the New-York Board were maintaining a non-intercourse with this city on account of its unhealthiness, that city was afflicted with disease, and the yellow fever itself, to a much greater degree than was known in Philadelphia. The weekly bills of mortality presented to them this fact in the most imposing form. The deaths in New-York, especially those from fever, exceeded considerably those in Philadelphia. The New-York returns, it is true, were vague and indefinite, numerous deaths being weekly reported as by "fever." Whether the physicians of that city are accustomed to make their returns of deaths with so much want of precision, in so loose and unscientific a manner, or that the particular denomination of fever was suppressed, is a very important and material point, that requires to be explained.* However that may be, that the general health of this city was far superior to that of New-York, that fewer deaths occurred from fevers, and that the fevers of New-York were quite as malignant as those of Philadelphia, are circumstances established by testimony and documents that are incontrovertible. Yet with that information daily presented to them, the New-York Board had not the magnanimity to acknowledge the truth respecting the existing state of things, but obstinately and unjustly continued to interdict the intercourse with Philadelphia. Was this a deception or a blind and partial reliance on the theoretical notions of the resident physician?

There is something mysterious in the proceedings of that gentleman. He did not hesitate to pronounce the fever that prevailed in Bancker Street, typhus. The learned professor, who so vauntingly boasts of his experience and deep research, cannot certainly be so little acquainted with the nature of typhus fever, as not to know that it never does appear epidemically in hot weather, and ceases on the approach of cold. All experience and all authority establish the reverse position as true, and typhus is admitted by the most eminent and skilful of the profession to be abated and destroyed by the heats of summer, and to flourish and be rife in the cold and damps of winter. Yet we cannot possibly suppose for a moment, that, animated by the intemperate zeal of a partizan, he should so far forget the respectability of his character, and the obligations imposed on him by his profession, as to attempt a deception on the uninformed; and, for the sake of a little popular fame, abandon and sacrifice the cause of truth.

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* See Appendix A.

Had the citizens of Baltimore acted as those of New-York have done, it would have been cause neither for surprise nor complaint. It might have been alleged, that they were justified from the previous conduct towards them in 1819 in retaliating the same on us. But how different were the spirit and feeling they manifested, and in how unfavourable a light do the sentiments and actions of New-York appear, when placed in contrast. In 1819 the yellow fever prevailed both in New-York and in Baltimore. The intercourse between this city and Baltimore was subjected to restraint, while that with New-York was kept open. Yet in 1820 New-York hastened, and seemed to embrace with avidity the opportunity presented by the appearance of a few cases of the fever in Philadelphia, which had favoured her the preceding year, to interdict the communication between the two cities. Very shortly after this step was taken, the fever broke out and raged in the heart of New-York, while it lingered and barely continued to appear in Philadelphia. Every means were tried in New-York to suppress a knowledge of the existence of the disease there, and the non-intercourse with Philadelphia was maintained, on paper, under pretexts entirely fallacious. The citizens of Baltimore, on the other hand, from whom different things were expected, and who might have supposed themselves entitled to complain and to retaliate, forbore to enforce restrictive measures, which they ultimately found were rendered wholly unnecessary. Let not these things be forgotten.

The proceedings of the New-York Board, on which I have animadverted, are an unpleasant topic. It was broached with reluctance and hesitation. But when high and important powers are conferred for purposes of public utility, and of a peculiar and interesting nature, the employment of which must be attended with serious injuries, and can only be justified by urgent necessity; when such powers are exercised with imprudence, and persevered in with pertinacity, perpetrating mischief without producing any benefit, silence would be criminal. The public discussion and examination of the manner in which they have been employed, more especially when a well founded suspicion may be entertained that they have been abused, becomes a duty. I do not mean directly and positively to charge the Board of Health of New-York with having established the non-intercourse between the two cities, or being induced nominally to continue it after its establishment, from motives of commercial rivalship and jealousy, with a view to prejudice the trade and commerce of Philadelphia, and benefit that of New-York. But, I believe, it will not be denied, that as their conduct now appears, they have fairly raised a justifiable suspicion that such motives, or others of similar character, might have influenced their proceedings.

Localities and Sources of the Disease.

The specific causes of general diseases, to which they owe their peculiar characters, are confessedly involved in much obscurity. Of their nature, the manner of their production, the properties they possess, and the laws that govern them, we are almost entirely ignorant. Not subjected to the evidence of our senses, known only by their effects, and those effects themselves not rightly understood, it is not surprising that great contrariety of opinion and observation, both of which to a certain extent must be conjectural, should prevail with different physicians.

On no subject more than this, ought medical men to be guarded against an overweening confidence in the correctness of their own opinions. The sources of error are so numerous, the probability of deception so great, false observations so prevalent, and the chances of accuracy so few, that he must be indeed highly gifted or peculiarly favoured, who can clearly perceive and accurately distinguish, amidst this cloud of obscurity, the true from the false.

With respect to the origin of yellow or malignant fever, in the cities of our country, opposite opinions have been and are entertained, both among medical men and the public generally. Those opinions have been embraced frequently on partial views and with limited information; have been sustained with ardour, zeal, and warmth of feeling, by their respective partizans. In this conflict of hostile sentiments, truth, it is to be feared, has been considered of less importance, than the fate of a preconceived doctrine; has been overlooked, and either partially or wholly suppressed, when its development would tend to shake a favourite hypothesis.

As to the sources of the disease that prevailed the past summer and autumn in our city, I have endeavoured to investigate them with as much impartiality as possible, and to relate them faithfully. All the circumstances which appear, or were supposed to have had any agency in occasioning the disease, in the different situations in which it existed, will be detailed, and the inquiry be pursued in the order in which the disease appeared.

I. *Hodge's Wharf.* Those persons, who adhere to the doctrine of the exclusive importation of yellow fever, have attributed the disease at this place to the brig Susan. Numerous stories respecting that vessel were in circulation during the period of alarm, of which few had the slightest foundation, and most were entire fabrications.

Having paid particular attention to the investigation of all the facts connected with this vessel, so far as she could be concerned in the production of the disease, at the time of its first appearance, I am able to present them in an authentic shape.

The brig Susan arrived at the Lazaretto July 2d, from St. Jago de Cuba, which place she left June 7th, having a passage of 26 days. Whilst at St. Jago, two of her crew died, one on the 12th, the other on the 20th of May. Both were sick and died on *shore*. The diseases were said to be gravel and yellow fever.

On the 15th of June, eight days after the departure of the brig from St. Jago, Mr. Geisse, a passenger, died on board, from a fever with which he was attacked the day after the sailing of the vessel, and which had been excited by violent exercise in gunning, whilst the brig lay becalmed. It is very certain, his disease was malignant fever. The bed, bedding and clothing, that Mr. Geisse had made use of, were thrown overboard, the cabin cleansed, and sprinkled with Cologne water.

On the arrival of the brig at the Lazaretto, the quarantine master was directed to have her cleansed and purified, which order, he reported to the Board of Health, had been complied with. On the 10th of July, eight days after her arrival at the Lazaretto, thirty-five days after leaving St. Jago, and twenty-five from the death of Mr. Geisse, permission was granted for the brig to proceed to the city, and the next day she left the Lazaretto.

The Lazaretto physician has informed me, that during his residence at the Lazaretto, twenty days from the time of a death or sickness on board of a vessel, has been considered by former Boards of Health, a sufficient time to test the healthiness of a crew and vessel. In the instance of the Susan, the time was extended to twenty-five days; a longer period, the Lazaretto physician has stated, than had been deemed requisite with any vessel under similar circumstances, for the last four years.

The Susan reached the city on the 11th of July, and hauled to at Pratt's wharf, adjoining to Hodge's dock, where she discharged her cargo, consisting of sugar and molasses, and left it on the 14th, remaining only three days. She then proceeded to the lower part of the city, where she lay during the summer.

The hold of the Susan was represented by Mr. James Bell, who assisted to unlade her, as perfectly clean, and the Health Officer who examined her made a similar rereport.*

The crew of the Susan, on her arrival at the Lazaretto, consisted of eight men, none of whom were subsequently sick of the fever.†

A stevedore and eight men were employed to discharge the cargo, none of whom became affected with fever or any other disease, and of the owners of the vessel, and some of their friends to the number of between twenty and thirty, who visited the brig,‡ the custom house officers and coopers employed on board of her, not one was taken sick. The number of persons, who had communication with the Susan, and consequently were liable to have contracted the disease from her, if she had been an infected vessel, must have been between forty and fifty, all of whom however remained in health. On the other hand, after the most diligent inquiry, it could not be ascertained, that a single individual, who was sick in this *locality* of the fever, had been on board the Susan, or had directly any communication with her.

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* See Appendix B. and C. † See Appendix C. ‡ See Appendix D.

The circumstance which led to a suspicion, that the disease had originated with the Susan was, that Jackson, who was the second person affected with it (considering Hays as the first), had worked in the sail loft of Messrs. Keen and Drais, in which one of the sails of the brig Susan had been placed, in order to be repaired. It was generally reported, that Jackson had assisted in taking the sail from on board the Susan, and in carrying it to the loft. Mr. Drais, who aided in removing the sail, positively contradicts this story; and what proves it wholly untrue is, that Jackson was not employed by Messrs. Keen and Drais, until the 21st of July, whereas the brig left Pratt's wharf on the 14th, and the sail was taken into the loft on the 11th. Jackson himself, to my repeated inquiries, always assured me, he had not been on board any vessel in a short time previous to his illness. It was also reported, that Jackson had worked on the sail, and had thus contracted the disease. But the fact is otherwise. The sail, at the time Jackson was taken sick, lay furled in the loft, precisely in the state in which it was, when placed there, and had never been touched by him, according to Mr. Drais' statement.* I examined the sail, and found it perfectly sweet and clean. Its history will be found in the examination of captain Smith, by the Lazaretto physician,† which shows the improbability of the sail's being infected. There were employed in the sail loft of Messrs. Keen and Drais, nine hands including Jackson, of whom Jackson alone was sick, and in going and returning to and from the loft, he daily passed and repassed by Hodge's dock, where the other cases of the disease principally existed.

Thus it appears, that none of those, who are known to have worked on board or visited the Susan, amounting to between forty and fifty, were subsequently ill with the fever; and none of those, who had the fever, are known to have been on board of her.

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* See Appendix E.

† See Appendix F.

I shall now proceed to point out the local causes, which may be supposed likely to have produced the fever in this situation.

1. *Hodge's Wharf and Dock.* The wharf on the north side is bounded by a range of frame buildings, which were crowded with inhabitants, and in front of which runs a gallery. It extends westward to the buildings on Water Street, which are occupied as stores, and there is a passage to it from Water Street, by an arched way. The wharf forms a square, closed on the north and west by lofty buildings, and is open to the east and south. It is consequently exposed to the direct and reflected rays of the sun, from an early hour in the morning until late in the afternoon. The wharf near to the dock is higher, than where distant from it, so that the water cannot drain from the wharf, but stagnates and evaporates on it. The people residing in the frame buildings which have been mentioned, some of whom were washer-women, were in the habit of throwing all their kitchen water, offal matters, and soap suds from the gallery on the wharf, and on a pile of plaster of paris, that had lain there for two or three years. The pile of plaster had in this manner become a receptacle of filth, which filled up its interstices, and the wharf was kept in a foul and offensive condition.

2. *Hodge's Dock.* This dock has been neglected for some years, and has gradually been filling up. At low water, it is at present uncovered nearly its whole extent, and a large mass of mud, of animal and vegetable remains, are thus exposed to the action of the sun and air. Two culverts, or tunnels, into which empty the privies of the range of frame buildings on the north of the wharf, and those of some of the houses in Water Street to the south of it, discharge their contents into the dock; and the inhabitants of that part of Water Street, few of whose dwellings had privies, are accustomed to throw into it, the contents of the buckets,

&c. which are employed as substitutes for those indispensable conveniences.

On the wharf south of the dock, there formerly stood a large frame building, which had been used for packing and storing hay, of which considerable quantities had fallen into the dock at different times, and when the store was burnt down about two years since, some hundred weight were thrown into the dock. This statement was made to me by Dr. Kughler and Messrs. Pratt and William Montgomery, whose counting-houses are in the vicinity.

3. In the month of May, a quantity of potatoes were landed on the wharf north of the dock, which were in a damaged state, and were extremely offensive. They were stored in the neighbourhood, where they were picked, and the worst of them thrown into the river, a few feet above the dock, into which a large portion was carried by the current, to add to the mass of decaying and putrescent matter already deposited there.

How long the potatoes remained stored in this neighbourhood has not been ascertained, but Mr. Joseph Lefevre, of the Union Line of Packets, has furnished me with some memoranda he made last summer, in which he remarks, that in "the latter part of June the smell from potatoes on "Race Street wharf, was so offensive, that people in Race, "above Front Street, could not stand at their doors."

I have thus detailed in an impartial manner, and I believe with correctness, all the facts that have a connexion with the appearance of the disease in this situation, both as it respects its foreign importation or its domestic origin. I shall now proceed to investigate, in the same manner, its causes at,

II. *Walnut Street Wharf*. The disease has been attributed in this situation to infected vessels and to local causes. There appeared for some time to be a difficulty in selecting the vessel, which had occasioned the mischief. At one time the sloop Isabella was accused of having introduced the disease. She is the vessel that was put under quarantine at New-York the last of June, in consequence of having come from Baltimore, where it was falsely reported that the fever had broken out. But as no fever prevailed in Baltimore, and as none of the crew of the Isabella had been sick, the supposition was abandoned. She arrived at this port June 28th.

When three of the sailors of the brig Martha were sent to the hospital, the rumour ran, that she had brought the fever to Walnut Street. But on investigation, it was found she had not been higher up the city than Pine Street wharf, where she had lain between two and three weeks, and had then been removed to Queen Street wharf in Southwark. It was ascertained subsequently at the hospital by Dr. Rhees, that the sailors of the Martha, had been at Walnut Street wharf about the time the fever commenced there, and had slept a night in one of the taverns in that neighbourhood.

The sloop Hector, as far as I have been able to learn, is now the only vessel to which suspicion of introducing the disease is attached. It was reported very generally, that the greater part of the crew of that vessel, had died in the West Indies, or on her passage home, and that she had put into Wilmington, Delaware, to avoid quarantine. Mr. Henry Cope, of the house of T. P. Cope & Son, who took great interest in the investigation of the causes of the disease, has favoured me with the facts concerning this vessel,* which he obtained from Mr. John Hemphill, to whom the Hector was consigned, in order to be sold. From this statement it appears, that the Hector arrived at Wilmington, Delaware, where she was owned by Mr. Baily, the latter end of June, from Cape Henry, and discharged her cargo. She was washed out and cleansed, and after remaining twelve or fifteen days, took on board a quantity of corn meal, which was consigned to Messrs. Masden & Buncker, and which was discharged at their wharf. She then fell down to the first wharf below Walnut Street. None of her crew were sick at Cape Henry, on her voyage home, or afterwards, and her original crew all returned in her.

I have not heard any other than the above vessels, accused of having introduced the disease, and the relation of the facts respecting them, will, I am confident, be found accurate.

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* See Appendix, F.

The local causes to which the disease has been attributed in this situation are as follows:

1st. A quantity of damaged vegetables, which were stored below Walnut Street wharf, especially beans and potatoes.

In consequence of the failure of the potatoe crop, the preceding year, in this part of the country, the importation of that vegetable had been unusually great. Such quantities, it is believed, never were before brought to this port. A very considerable part of what was imported, were on their arrival, in a very bad state, and some cargoes completely damaged. The greater part were landed and stored at Walnut Street wharf. Mr. Lefevre in his memoranda, made last summer, states, that " it is a well known fact, that large quantities of potatoes have been stored in the neighbourhood of Walnut Street wharf, where they have been kept in a very confined state, and also kept in several dwelling houses, whose occupiers are, or have been in the practice of keeping on hand all kinds of vegetables, for the purpose of supplying shipping, and have dealt largely in potatoes, particularly in the early part of the summer, at a time when they were landed from vessels in a damaged state." In the same memoranda, it is stated, " that the schooner Alert, Capt. Cobb, arrived and hauled to at Beck's wharf in July, with six or seven hundred bushels of potatoes, which were in a damaged condition. When the hatches were taken off, a thick and offensive vapour issued from the hold, and so unpleasant was the smell, that it caused several counting-houses in the vicinity to be shut. Those potatoes were stored in the neighbourhood of Walnut Street wharf. Two negroes were employed in the hold of the schooner, to shovel the potatoes into barrels, to be hoisted on deck and delivered to carters. One of the negroes has not since been seen, and it is supposed he must have taken sick and died. The other was attacked with fever immediately after the completion of the work, from which he recovered with difficulty; Capt.

Cobb sickened from the effluvia from the hold, and one of the sailors was also ill, but both recovered after seven or eight days illness." On the 29th of July those potatoes were hauled to the commons and thrown away.

It has been supposed, from the great quantities of damaged potatoes, that arrived in the spring and the commencement of the summer, and which were purchased by the blacks for a trifle, or were given away to them to feed their hogs, that the fever which afflicted those people, must have been produced from that cause. Certain it is, that in many of the places where that disease was prevalent, considerable quantities of damaged potatoes had been kept by them.

In the store immediately in the rear of Messrs. T. & C. King's counting-house, one of whom was the gentleman who was taken sick in New-York, and removed by the order of Dr. Hosack, at noon, in a boat across the bay, without a covering to defend him from the rays of the sun, were stored in the month of June twenty-five hundred bushels of potatoes, which were in a damaged state. They became so very disagreeable to the neighbours, that they were removed in the course of that month. Mr. King and Mr. Duffy have both informed me, that when the store was opened, a dense, offensive vapour rushed out; it seemed, they stated, as if the store was on fire. The store was washed out after the removal of the potatoes; but the offensive smell still continued.

The winds at this time, were almost constantly from the southward. The store adjoining that in which the potatoes were, and the house in Water Street on the same line with it, were the south limit of the disease in this situation. It extended north from this position to Tun Alley, less than a square, or about seven hundred feet. From the Messrs. King's store to Walnut Street, and the same distance along Water from Walnut Street, forms a square of about one hundred and fifty feet. Within fifteen days from the removal of the potatoes brought by the Alert, and ten from the commencement of the disease, between twenty and thirty cases of the fever occurred in, or were traced to that square.

2d. The dirty and foul condition of the wharves in this situation. Immediately above Walnut Street large quantities of molasses had been landed, the hogsheads of which were in a very bad condition, and in consequence had leaked considerably. From the frequent showers that fell, the wharves were moist and soft, and the molasses became worked up by the constant passing and repassing of carts, drays, and people, with the mud, which is a compost of various putrefiable and fermentable matters. In this moist and lutulent state the wharves remained for some time, exposed to the fervid beams of a July sun. The putrefactive fermentation was thus occasioned, and a most noisome effluvia was perceptible in this situation.

3d. The pavement of an alley immediately above Walnut Street, leading from Water Street to the wharf, was taken up for the purpose of repairing, and the old earth, which is altogether in this situation made ground, was turned up, and left exposed some weeks to the action of the intense heat of July.

The above comprise the local causes, that were conjectured to have an agency in the production of the disease in this quarter. How far they were adequate to occasion it, and the probability that they were the agents in accomplishing the evil, those accustomed to investigations of this nature will be able to determine.

III. I know not whether the two cases which occurred at Mr. Anderson's, in Water Street between Market and Arch Streets, are to be considered as sporadic cases, similar to a number of others which afterwards appeared in various parts of the city, and which could not be traced to any of the original seats of the infection, or to any communication with the sick, or that the disease was produced by the cause I am about to state.

The schooner Lydia and Mary, captain Shippen, from Port au Prince, lay at the wharf directly in the rear of Mr. Anderson's house. She arrived at the Lazaretto from Port au Prince, after a passage of eleven days. The crew being in health, and having continued so during the voyage, the port from which she arrived being also healthy, she was permitted to proceed to the city. Her bilge water was pumped out after she was moored to the wharf, and was very offensive. Being sick at the time of the occurrence of these cases, I was not able to ascertain whether any other local causes of disease existed in the vicinity. No other persons were affected with the disease, which seems to cast a doubt over the production of these cases from any extensive local cause. Neither Miss Anderson nor the clerk had been where there were any persons sick.

IV. *The Families of Wharton and Drinkwater.*—It was at first supposed, that the disease which proved so very destructive to these families, was derived from the same sources that gave rise to it at Walnut Street wharf, as their houses opened on Water Street. Mr. Wharton, a short time after the mournful event in his family, informed me that Drinkwater had in his cellar and vault, which opened into the street by a grate, a quantity of damaged potatoes and putrid fish. Wharton's children frequently complained of the noxious effluvia emitted by them. When the disease at Walnut Street wharf began to attract attention, Miss Drinkwater requested Mr. Wharton's family to join with her in remonstrating to her brother against keeping these articles any longer, as the fever was attributed at Walnut Street to damaged vegetables. In consequence Drinkwater had them removed. In less than a week afterwards all the members of those unfortunate families that perished, were ill with the disease. The grate of Drinkwater's vault is directly opposite Norris's Alley, in which about the same time several cases of the fever occurred.

V. *Letitia Court.*—It has been conjectured, that the disease was brought to this situation, by a female, who took the disease at James Forsyth's, in Water Street near Walnut. The facts are as follow. Mrs. Townly attended on Forsyth, the night of the 9th August. She was taken sick on the 13th, and died on the 17th. By a mistake, ten grains of corrosive sublimate were administered to her instead of calomel. The stomach was very irritable and instantly rejected the dose. It is now impossible to decide, whether the symptoms and fatal termination of this case, was produced by the powerful poison she had taken, or whether it was truly a case of malignant fever. The symptoms that result from poisons of this nature, are scarcely to be distinguished from those of malignant fever. The case, however, was considered as one of malignant fever, and treated as such. The night on which Mrs. T. died, the bed, bedding, &c., were sent to the Lazaretto, the room was fumigated, white-washed and scrubbed, as was done in all similar cases. On the 2d of September, Martha Prichett and Eliza Curtis were taken sick. They lived at Basett Baker's tavern in the court, about one hundred feet distant from the building in which Mrs. Townly died, but several houses intervened between Baker's and Mrs. Townly's. About the same time a young man who worked as a journeyman in a hatter's shop, in Market Street, five houses below the corner of Letitia Court, was attacked, and on the 7th Robert Bancroft, an apprentice to Mr. Sexton, the next house to the corner of the court, was also taken ill. The room in which Mrs. Townly was sick, has a dead wall, without an opening towards the houses below it. Neither of the young women, the journeyman hatter, nor Mr. Sexton's lad, had any intercourse or communication with Mrs. Townly or her family.

There were no local causes, that could be particularly designated, as having given rise to these four cases. Several of the cellars of the houses in the court, and Baker's was one of them, were used by the country people and hucksters who attend the market, to keep their vegetables, cheese, &c. in, from one market day to another; but I could not learn that there had been any in a damaged or putrefactive state.

VI. *Second Street near Shippen.*—It is difficult to account satisfactorily for the appearance of the disease in this quarter. So far as I could ascertain, there were no obvious local away, particular attention was paid to remove his bed and bedding, which were destroyed; and the house was washed and fumigated the same morning.

The second case that occurred was Mrs. Brewer, who lived in a different street, and distant from Lodor in a straight line, between three and four hundred feet, and with a number of houses intervening. The times and manner in which the other cases occurred, are conclusive, that the disease could not have been communicated from one to another. Two persons, on the night of the 18th of September, were attacked, living about two hundred feet distant from each other. On the 19th, the weather became cool and wet, and continued so during the 20th; and on the night of that day, five cases occurred, scattered over a space, the two extreme points of which, north and south, were distant about three hundred yards, and which extended from east to west about one hundred feet; and on the night of the 22d, two more were attacked in the same limits, living about one hundred and fifty feet from each other. Thus in the space of four days, a spell of cool weather developed the disease in nine individuals, most of them living apart at considerable distances from, and only two of whom had communication with, each other, or with any sick person previous to their illness. The cases of Mrs. Hand and Miss Keen, are particularly striking. They resided in Green Street, the most remote point to which the disease extended north, removed from the nearest person sick to them when first taken, about two hundred feet, and had lived for several weeks very retired. Capt. Hand informed me, they had not been out of the house, except to church, for two or three weeks prior to their illness.

The circumstances attending these cases, most decidedly prove, that the disease was not and could not have been carried into that quarter by young Lodor; and could not and did not spread from one to another, by personal contagion. The only inferences that are authorised by the facts, are, that the disease was caused in those who were attacked with it, by a poison floating in the atmosphere, which, unhappily, they inhaled to such an extent, as to suffer from its deleterious operation, or had systems peculiarly susceptible to its effects.

I will now proceed to show what were the local causes, which may be conceived to have been sufficiently extensive and fully competent to the generation of a subtle poison, which disengaged into the air, was capable of producing the fatal and terrible disease, that ravaged this neighbourhood, when introduced into the human system.

In the first week of September, a memorial dated August 29th (the day Lodor was taken sick, but which was then unknown to the neighbours), signed by eighteen respectable citizens, residing near Pegg's Run, was sent to the Board of Health, directing its attention to, and requesting its interference for, the suppression of "the greatest nuisance in Philadelphia." That nuisance was Pegg's Run, which had been dammed up at New Market Street, by the erection of a bridge. The memorialists in describing the evils that were then experienced, predicted that disease must be the consequence of its continuance, little expecting that their fears were so soon to be verified.*

Pegg's Run is to be considered as an open culvert or common sewer, passing through the closely built parts of Penn Township, Spring Garden, and the Northern Liberties, to the river Delaware. In its course it receives the contents of the gutters of the numerous populous streets and alleys it crosses, and two culverts from the city also open into it. Along its borders are situated a number of manufactories of glue, starch, dressed skins, and soap; about fifty slaughter-houses, and the privies of most of the adjoining dwellings, the refuse, fermentable and putrescent matters of which are all emptied into its stream. Except during heavy rains, or immediately after them, the stream is barely sufficient to carry along, with a sluggish current, the mass of decomposing offensive substances that compose it, for in fact it seems more like liquid mud than water.

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* See Appendix G.

By the erection of a bridge across Pegg's Run at New-Market Street, last spring, a stagnating pool of the feculent stream just described, was formed west of the bridge, from two to three hundred feet in length, from twelve to fifteen in width, and about three deep.* In the months of July and August, this pool was exposed to the action of the sun and air, while the thermometer in the shade was daily above eighty degrees at three o'clock, and sometimes as high as ninety degrees; circumstances in the highest degree favourable to the production of the most noxious exhalations.

From Pegg's Run, on the north, the ground rises with a gentle ascent, on the crown of which is Duke Street, distant between three and four hundred yards from the run. New-Market Street crosses Pegg's Run at the bridge, and terminates at the rear of the lots on Duke Street, making a large avenue leading from the creek to Duke Street. In the intermediate space there are but few dwellings. The winds, at the period abovementioned, were generally from the south or southwest, and an inspection of the whole ground will show that Duke Street, east of New-Market, is in a direct line in the course of the prevailing winds from the pool west of the bridge, and is the first place that would arrest the exhalations from it, when wafted by the currents of the air usual at that time. It was precisely at this place that six of the twelve cases occurred, all of them in the space of about one hundred and fifty or two hundred feet. The three cases in Green Street were on an open lot, which communicated with Duke Street at this point, about one hundred feet distant, and the other three cases were nearer to Pegg's Run.

As the disease in Duke Street and vicinity, must be referred either to young Lodor having carried it thither, and communicated it to the others who were affected with it, or to Pegg's Run, in the peculiar state in which it was, exposed to, and operated upon, by an atmosphere heated to a tropical temperature, the facts which are now detailed will enable those who are interested in this subject to decide for themselves the more probable source by which it was produced.

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* See Appendix H.

Classes and Symptoms.

The same disease will frequently present different characteristic distinctions, according to the greater or less intensity of the morbid action, and as more or fewer of the vital organs become involved in the derangement of the system.

This difference in the symptoms does not proceed from, or indicate a specific difference of action, but constitutes different grades of the same action; which, occurring frequently and regularly, forms natural classes or divisions of a disease, an attention to which will aid in illustrating its nature, lead to a just prognostic and correct treatment, and should never be neglected by the practitioner.

Small-pox presents a striking exemplification of this observation. The same specific action, from a difference in force, and from a greater or less extensive affection of the system, produces the very dissimilar train of symptoms accompanying the distinct and confluent disease.

Intermittent fever is doubtless the same specific disease, whether it be a quotidian, tertian, or quartan; but a difference in the intensity of impression and action, and in the extent of associated or sympathetic motions, give rise to a concourse of symptoms, which, invariably accompanying that particular state of action, &c., mark them with well defined boundaries, and constitute a particular and peculiar class.

Since the ample and enlarged experience acquired from the prevalence of typhus as an epidemic in England, Ireland and Scotland, the late writers on that disease, especially Dr. John Armstrong, who has written on it with great research, accuracy and closeness of observation, have divided typhus into three classes; the simple, inflammatory, and congestive, each distinguished by peculiar features, which, though bearing the impress of a family likeness, still admit of correct discrimination.

Plague, by most writers who treat of it, is divided into different classes, each designated by certain groups of symptoms. Dr. Russel makes no less than six classes of that disease. Sir Brooke Faulkner, who is considered as the best modern authority, and whose arrangement has been adopted by Dr. Armstrong, reduces them to three divisions.

Malignant or yellow fever having a close affinity to, and standing in the same rank of diseases with, typhus and plague, it may with propriety be expected to present a similar character with those diseases in this respect. Dr. Physick, whose opportunites for observing this formidable disease in some of our most terrible epidemics, were extensive, and whose accuracy of observation is not excelled, I have understood, had remarked, that three grades of the disease could be evidently distinguished, differing in the concurrence of their symptoms; and Dr. Parrish, whose experience has rendered him familiar with the disease in all its forms, has informed me, that he was able to make the same discrimination.

The disease, as it occurred in this city in the last summer and autumn, was easily distinguishable into different grades, and presented well marked divisions, which indicated a difference of treatment, and constituted the basis of a different prognosis. In describing the symptoms of that disease, I shall, therefore, consider them under different classes, and as characterising those classes.

Class First.

The cases comprehended in this class display the disease in its greatest violence, and justly entitle it to the term Malignant Fever.

It was sometimes preceded with general languor, and sense of debility for a short period; in a few instances, these feelings occurred suddenly, while the patients were walking in the street, and produced staggering as from drunkenness, to such a degree, that it was with difficulty they could reach home. The disease in many cases commenced without any previous warning or preceding symptoms, passing rapidly from perfect health to a state of mortal danger. In a great majority of the cases, it commenced in the night. Writers on this disorder narrate instances, where the patient was struck down instantaneously, as with a blow or stroke of lightning, while walking or otherwise occupied, and becoming immediately comatose. Drinkwater's case exhibited this malignant state of the disease. He was apparently slightly unwell in the morning, but denied to Dr. Worthington, who was attending Miss Drinkwater, that he was indisposed. Early the next morning, I received a note from Dr. Worthington, dated at eleven o'clock the preceding night, informing me he had just been called to Drinkwater, who was violently attacked and was without any attendants. I immediately repaired to his dwelling, but found him dead. He had just expired in a strong convulsion, the muscles being still rigid, hard, and contracted with spasm; his skin yellow, and black vomit issuing from his mouth.

Chills, with few exceptions, occurred at the beginning of the attack, and were frequently distinct rigours; but sometimes amounted only to a sense of coldness or a crawling sensation. In some cases, and those of the greatest violence, they were entirely absent. Miss Anderson, Dr. Parrish informed me, had no chill, nor had young Sharpe, whom he attended. I noticed the development of the disease in Miss Jane Mann, which came on instantly and without a chill or feeling of coldness preceding it. She had returned but a few minutes before with her mother, from witnessing the interment of her elder sister, and had been alarmed and agitated at the report of a gun fired with a view to raise a drowned body. I noticed that she reclined her head on a table, and inquired whether she felt indisposed, which her mother disbelieved. On examination I found the disease completely formed. Her eyes were inflamed; the face was flushed, the skin hot, pulse quick and frequent, and she said her head had commenced aching. In four days she was a corpse.

The blood-vessels of the eyes were uniformly injected with blood, and were little affected by purging, local, or general bleeding; but on the third or fourth days, they became still more numerous and highly distended, and the adnata assumed a yellow colour or a dusky hue. In some few cases the adnata appeared to be a mass of blood-vessels gorged with blood. The eyes at the commencement were often watery, and had an expression similar to that of drunkenness. A wild fiery look generally indicated convulsion. A dull obtuse pain was felt in the globe of the eyes, and sometimes complaints were made of a pricking sensation, or a feeling of compression and tightness in them. A fixed steady stare, the eyes perfectly immoveable, was often seen to precede dissolution.

Pains in the lower extremities were generally felt at the beginning, and during the first and second days. The back was also terribly afflicted with torturing pains along the spine, shooting forwards towards the umbilicus, and downwards towards the thighs. An obstinate, irremoveable, and distracting pain was seated in the forehead, in the region of the frontal sinuses, which succeeding instantly pains in the joints of the lower extremities, constituted, in some cases, the first features of the disease.

The state of the skin was various. In some it was pungently hot, dry, and harsh, from the commencement. In others it was generally or partially moist for the first and second days. In one case I found profuse perspiration until the close of the second day, when the skin became dry and cool, attended with a complete torpor of its vessels and loss of its irritability. Hot bricks, bottles filled with hot water, and the decoction of cantharides in spirits of turpentine, applied boiling, were not felt, nor did they produce the slightest impression. Most of the cases of this class were attended with an entire torpor of the capillaries of the skin, even when the heart and large arteries were acting violently. It generally was most observable about the third day, but was met with sometimes in the first and second. In some instances, from this cause, the skin assumed a dark, mottled, and dusky hue, giving the complexion a mahogany tinge, proceeding from the languid circulation of the capillaries, permitting in them the accumulation of dark or partially oxygenated blood. This lentor of the capillaries was rendered very evident by pressing on the skin. The spot from which the blood had been forced would remain pallid some minutes, when it very slowly and gradually resumed its former colour. Depending and projecting parts, such as the scrotum, fingers, toes, and ears, were, from this cause, often very dark and discoloured, especially a short period before dissolution; and immediately after death, the blood, which always remained fluid, settled in them, and along the back and neck, giving them a dark purple hue. When the skin did not put on this dark appearance, it became of a yellow colour of greater or less intensity. This tinge began most generally about the third or fourth day; but in some instances was not perceptible until about the period of dissolution, and became deeper after death. It was not confined to the skin alone, but pervaded the fat, cellular membrane, and even the periosteum of the bones. The perspiration was, in some cases coloured, and dyed the linen of the patient, and white handkerchiefs, of a bright saffron hue.

The insensibility and inirritability of the skin was, at times, so great, that they could only have proceeded from the death of the surface, while life existed in the centre of the system only from a narrowed and partial circulation throwing the blood upon the heart and large arteries, and thus maintaining their action. Mr. Philips has shown, in his experiments, that when the circulation is limited and confined, the action of the heart and large arteries will continue under circumstances, that would otherwise occasion instant and universal death of the system. Thus, after securing the carotids and large arteries of the extremities, he decapitated dogs, and amputated all their limbs, and by inflating the lungs, kept up the action of the heart and the aorta for several hours. In the same manner, the complete death of the capillaries, forcing and confining the blood in the large vessels, their action was continued until, upon the entire cessation of the function of the brain, respiration, which is under the cerebral influence, terminated, and the blood was thus deprived of that property by which it is enabled to stimulate the heart.

I have seen, in several cases, the patient remaining from twenty to forty hours, in a comatose state; the senses obliterated; the extremities and surface cold, and all its functions destroyed. I have had turpentine and cantharides poured when boiling on the abdomen, and rubbed on the extremities for hours, and yet the slightest redness was not produced, nor any more effect, than if it had been applied to a statue. From every appearance, animal life was wholly extinguished, and mere organic vitality remained. The heart in such cases would sometimes act with great force. In one, its action and that of the carotids, could be distinctly seen across the room, every pulsation shaking the patient's frame, though a robust man. A most intolerable fetor proceeded from this person's body for twenty-four hours before his entire and complete dissolution.

The tongue was covered, in every case I saw, with a coat of fur. In the commencement it was moist, of a cotton white, sometimes so delicate, as to seem as if covered with a piece of fine muslin. It continued in this state frequently until death, when it occurred about the fourth, fifth or sixth days. In some, however, towards the termination, when fatal, it became foul, dry, and loaded with sordes; sometimes a dark streak occupied the centre, and the edges were white; it was always a bad symptom. Writers describe it as perfectly natural in this disease. I met with no instance of it last summer.

Soreness of the throat was occasionally a symptom, and in two cases I attended, there was a total loss of deglutition, and an inability to vomit for several hours before death.

The stomach always evinced how largely it partook of, and how deeply it was concerned in this fatal disease. This has emphatically been said to be its "seat and throne."

Tenderness of the epigastrium is so very prominent a feature in malignant or yellow fever, that it has been considered as a pathognomonic symptom. It was not, however, so universal an attendant on the disease last summer as I had expected to find it. It is singular, that the cases which occurred near Hodge's wharf, should have been all nearly free from it. It was generally very slight with them. Pressure made with force on the epigastrium, was borne without difficulty, though the stomach exhibited, by other symptoms, its highly disordered condition. Vomiting and violent straining to vomit, commenced very early, and could with difficulty be controlled. It ceased, in several, some hours before death. Most generally, in the commencement, nothing but the drink and other liquids that had been swallowed were thrown up; or a clear glazy mucus. Bilious vomitings, which are mentioned by most writers, I did not once meet with in this disease. From the extreme irritability of this organ, the smallest doses of medicine were frequently productive of great distress, and would be instantly rejected. One of my patients absolutely refused on the third day to swallow any thing, not even a table spoonful of water. He kept his teeth firmly closed, and spit out whatever was forced into his mouth. The last words he spoke was a declaration, that he would rather die than swallow. Another, when I pressed him to take a mild cordial, told me he could not swallow, it was like putting daggers into his heart. In both these cases, the stomach was distended with a fluid, the fluctuation of which was distinctly heard on every movement. After death, it was found filled with black vomit, though none had been thrown up during life.

Various distressing sensations were experienced in the region of the precordia, such as oppression, great weight, burning, and a feeling of distention.

The respiration in some, was very laborious and hurried; in others, slow, accompanied with deep and heavy sighing. Miss Bechtel, in Duke Street, suffered from a feeling of suffocation, which she ascribed to an impossibility of inflating the lungs; it was accompanied with violent spasmodic pains of the chest, attended with a red streak, about a finger's breadth, along the sternum, which disappeared on the subsidence of the pain.

Black vomit was the fatal symptom, that attended on and marked the fatal termination of the disease in more than two thirds of those that died. Not a single recovery took place after its appearance. It was found in several cases, besides the one already stated, on examination after death, when it had not been ejected during the course of the disease. It appeared, in these cases, as if the stomach had lost its vitality to so great a degree, that its irritability had ceased.

Hiccough was an occasional occurrence prior to death; but it was also present in those who recovered, and in one case continued forty-eight hours.

The bowels, in the greater part of the cases, were obstinately costive; and, when evacuations were procured, they did not always give the relief that was expected from them. In some cases the skin became cooler and the pulse slower, after the operation of purgatives, but without any abatement in the malignant expression of the general symptoms.

Hæmorrhages from the gums and lips were frequent, both in those who recovered as well as those who died. In one fatal case it took place from the uterus.

The urine was always deficient in quantity, was generally of a deep red, but imparting a yellow colour to the linen, and deposited a heavy and copious sediment. In some instances the discharge of it was entirely suppressed. This symptom arose from a paralysis of the bladder in some cases, as it was found after death to be distended with urine; but in other cases, it was occasioned by a defect in the secretion. It was then invariably a fatal symptom.

The pulse, on the commencement of the attack, was increased in quickness and frequency, and in some cases in fulness, but which last often disappeared on the second or third day. It often became natural on the third and fourth days in fatal cases, and sometimes continued thus throughout the progress of the disease. A full, but very compressible pulse, the artery feeling as filled with air, and yielding to the slightest pressure, was met with in several cases. It indicated great danger. None recovered in whom it was present. I did not meet with a hard or tense pulse, nor have I been able to learn, that such a pulse came under the notice of any of our physicians. About the third and fourth days, in repeated instances no pulse could be felt at the wrist, though the heart and carotids were acting, and the muscular powers were undiminished.

On the third and fourth days, and sometimes at a later period, there was frequently an apparent mitigation of all the symptoms, which might easily deceive the unwary and inexperienced into a belief that a remission had taken place. In one instance, the patient, a lad, appeared so perfectly well, that his parents permitted him to leave his bed, and play nearly all the afternoon on the wharf; but next morning black vomit came on, of the consistence of tar, attended with most distressing retching and efforts to vomit, and at one o'clock the little sufferer expired.

Sharp hunger was felt by several a few hours previous to dissolution, and was a fatal symptom in every instance in which it occurred.

It was almost impossible to retain some patients in their beds, from an unconquerable feeling of restlessness with which they were afflicted. No position gave ease long, and a respite from torment was expected from a new attitude or a new resting place. This constant jactitation was a fatal sign.

The mind was variously affected. Fear was a common feeling at the commencement; but there was, with scarcely an exception, a perfect indifference as to their fate towards the close. Some retained their senses perfectly, while others early became delirious. In two instances it was almost maniacal. Some would be, as it were, in a deep reverie; and when spoken to, would start with surprise, and answer in a hurried manner. Many apparently labouring under great distress, when asked how they were, would answer very well, nothing ailed them.

The physiognomy had something peculiar and striking. It conveyed at once an impression of the malignant nature of the disease and the danger of the patient. It is impossible to describe the appearance of the eyes and features; the look and expression that seemed to indicate an internal consciousness of the mortal struggle in which the vital powers were engaged; or the [illegible] wl of a gloomy indifference that mantled the brow, [illegible] hrouded the countenance with the expression of a s[illegible] defiance of the fate, that seemed impending over [illegible] m with certain destruction: but once seen, it could not [illegible] forgotten; and the apparent calm and quietude that s[illegible] quently reigned exteriorly, was a diagnostic sign t[illegible] uly displaying the real nature of the malady, and f[illegible] ortending the fatal storm that was to ensue.

The symptoms I have thus detailed, with a degree of minuteness that perhaps may be conceived unnecessary, I witnessed in different cases that came under my notice, either in my own practice or that of others, or in the City Hospital. They were not all present in any one case, but more or less of them were exhibited, in every individual who was attacked with the disease in this aggravated form. Any number of those symptoms, taken aggregately, will, I think, characterise a class, in which the disease exists in its most malignant shape, and, generally speaking, transcends in its action the powers of any known remedy, and the resources of the best directed skill. We may truly say, in the language of Virgil,

> "cessere magistri,
> Phillyrides Chiron, Amythaoniusque Melampus."
> Geor. Lib. iii. 499.

In fact, as Dr. James Johnson remarks, the patient began to die with the commencement of the disease. Authors mention recoveries after the existence of many of the leading symptoms I have just enumerated, but I neither saw nor heard of any in our late partial epidemic. A large proportion of the cases that occurred were of this class; and consequently the mortality, in proportion to the whole number affected, was very great. Most of them terminated on the third, fourth, or fifth days. A few were protracted to the seventh and eighth.

Class Second.

The diseases in the cases of this class bore, in the commencement, a strong similarity to those in the first. But the diverging point soon became obvious, and permitted the discrimination between them to be drawn. Languor, lassitude, and a chill, most generally distinct, were the premonitory symptoms: reaction always occurred, when occasionally the excitement produced a flow of spirits, almost similar to a slight intoxication.

Pains in the limbs, back and forehead, were always present; the eyes were inflamed; the skin was hot and dry, or moist, and the face flushed; and approaching stupor or delirium often attended after the accession of the fever. But active purging, small general or topical bleedings, blisters on the back of the neck, and cold applications, either alleviated these symptoms, or evidently prevented their increase. An impression, it was obvious, could be made on the system.

The stomach was always irritable, and the vomiting often distressing to the patient and embarrassing to the physician. Bile would be sometimes apparent in the discharges, but it was not in such quantities as to sanction a belief, the secretion was very considerable. It was most probably merely shaken from the gall bladder by violent straining. The diminution of the vomiting on the third, fourth, or fifth day, was a favourable omen.

The skin and the eyes, became suffused with yellow in many instances from the third to the fifth day.

The pulse was never natural, but always disordered, being increased in frequency and quickness, sometimes bounding, and was possessed of considerable force. In one case it beat one hundred and sixty strokes in a minute, and was generally from one hundred to one hundred and twenty. It became slower, softer, and fuller, from the fourth to the sixth days, in those that recovered.

The discharges from the bowels were dark coloured and offensive. Medicine operated on them with ease, and the operation was always productive of temporary relief.

The skin never lost its sensibility and irritability, and thus kept open this favourable approach to a vigorous impression on the disease. Diaphoresis could most generally be easily excited by the vapour bath, conjoined to diaphoretics, and was of decided utility. A moisture and softness of the skin on the fifth, sixth and seventh days, was an indication of convalescence.

The urine was scanty, of a deep red, imparted a yellow stain, and deposited a copious sediment. But it was never suppressed.

A hæmorrhage from the nose, occurred in some cases on the fifth and seventh days, and appeared to be a critical evacuation. Hæmorrhage from the gums also occurred in a protracted case, in which the patient survived.

The tongue was in most instances slightly furred and moist. When the disease continued beyond the fifth day, it sometimes became dry and covered with sordes. Becoming clean at the edges, indicated a favourable change.

The disease in this class was evidently less malignant in its nature, and was in the range of remedial powers. The practitioner, if called in at an early period, and pursuing a judicious treatment, would, with few exceptions, have the satisfaction to find this formidable disease under his control, and subdued by the power of medicine. It was only when called in too late, when organic lesions had been produced by the long continuance of morbid impression, action, and associations, or early mismanagement, that he would experience the painful conviction, that all the efforts of his skill would be unavailing.

Class Third.

In this class, the disease, though possessing the same features, the same outline of symptoms as in the other two, was mild and easily manageable, yielding with great facility to the remedies employed. In some cases it was no more than an ephemera, terminating in a single day, from the operation of an active purge, a profuse sweat, or single bleeding. In others it continued from three to five days. When it continued that length of time, it was attended with *remissions*. In the first class there were no remissions: in the mildest cases of the second class, there were sometimes attempts at, or concealed remissions, if the term may be employed. And it was only in the cases of this class, that they were distinctly perceptible.

Chills, pains in the extremities, spine and forehead, but not so intense as in the other classes, were the commencing symptoms. The eyes were suffused, in some cases very deeply; the skin hot, dry, and harsh, the face flushed, the mind confused, the pulse quick, frequent, full, and the signs of an open or developed febrile paroxysm exhibited. The stomach was generally quiet; but, in the worst cases, was irritable, attended with copious vomiting, and bile was more frequently discharged, and in greater quantities, than in the cases of the second class. Any strong impression on the system, produced an immediate effect on the disease, and frequently dispersed it at once. No doubt, many of these cases, if left to themselves, would have recovered by the mere powers of nature.

It is a circumstance worthy of attention, that in the different quarters where the disease prevailed, one class would be more common than the others. Thus at Hodge's wharf, and Duke Street, nearly all the cases were of the first class; in Letitia Court they were all of the second; and in the other situations, the second and third classes were nearly as numerous as the first. Dr. Monges, whose accurate knowledge of this disease, derived from much experience and long observation, both in the West Indies and this country, is so well known and acknowledged, states that he has noticed the same facts, in the different epidemics of our city. In certain situations, when the disease was general, he found it light and easily manageable; all his patients recovered: in other situations, it was precisely the reverse; medicine was unavailing, and his patients nearly all died. This is an observation of great value; and the fact appears to be quite incompatible with the doctrine of contagion. Contagious diseases, when epidemic or otherwise, will differ in force, as it respects individuals, according to the state of the system or constitution of each, but never present a constant and permanent difference—being according to the situation, mild and simple in one place, and malignant and aggravated in another. Does not this fact render the supposition highly probable, that the poison which produces yellow or malignant fever, is evolved into the atmosphere, or collects in certain spots in greater or less quantity, or from some cause becomes more or less highly concentrated in them, and thus occasions different degrees of poisoning of the system? We observe similar effects to arise from the exhibition of poisons in different quantities. Thus a certain number of individuals may take a certain quantity of arsenic; they may all be more or less affected, but will *all recover* without difficulty; others may take a larger dose, which will produce a greater degree of disorder, and a more violent train of symptoms; but by timely and prompt applications, the larger proportion will recover; others, again, may receive a still stronger dose, which will occasion such organic lesions, that nearly all will fall victims to its deleterious operation, one or two only escaping, as it were by miracle. These effects bear a strong similarity to the different classes of yellow or malignant fever, the poison producing which, like arsenic, acts chiefly on the stomach, and produces a lesion of that viscus in proportion to the quantity introduced into the system.

(To be concluded in the next number.)

Art. VII. *Animadversions, by* Dr. Hare, *on the Review of his New Theory of Galvanism, published in the first number of this Journal.*

"Experto crede."

A REVIEW of my Theory of Galvanism appeared in the last number of the Philadelphia Medical Journal, in which it is either directly alleged, or indirectly insinuated, that I have proposed a theory more "obvious," and at the same time less tenable than those which previously prevailed; that I have deliberately published opinions which ought not to have been even casually suggested, and which it is presumed, must, from their absurdity, have been spontaneously abandoned; that while my opinions are contradicted by my own statements, my experiments prove the reverse of the doctrines in support of which I was at the trouble of making them; that my enlargement of the original galvanic pair under one form, is essentially the same as Wollaston's abridgment of it under another; that my apparatus of *two* galvanic pairs, without insulation, is a contrivance not quite so "obvious" as an "obvious remedy;" and that the principle of it, (since advantageously used in a series of eighty pairs) would prove ineffectual if extended. On the merits or rather demerits of my theory, the reviewer thus concludes: "Upon the whole we think it clearly proved, that mechanical electricity and galvanism are modifications of the same principle; and that of course the theory of Dr. Hare cannot be sustained."

In reply, I propose to show the assertion of the reviewer, page 158, that galvanic and electrical ignition "are entirely analogous," and the opinion that "mechanical concussion, especially when produced by an electrical discharge, cannot sunder atoms chemically combined," inconsistent with a practical knowledge of the subject, and contradictory to facts mentioned in some of the best treatises on galvanism. I allege also, that the insinuation that my opinions are inconsistent with my own statements, is rendered plausible only by displacing a sentence which restricted the meaning; that in citing Wollaston's experiments, the reviewer omits circumstances corroborating my opinion, and irreconcileable with his own; that he has fallen into a very great mistake, in imagining that when charcoal is subjected to a fluid supposed to contain caloric, the intense ignition of the charcoal proves that the caloric has not been arrested by it; that, had he studied my theory attentively, he could not have inferred that "electricity should have passed," where, *by the premises*, heat only could have been indicated, nor have appealed to the properties of heat alone to disprove an hypothesis founded on the properties of heat and electricity combined; that the event has disproved his prediction, that the principle of my voltaic apparatus of two pairs, would not bear extension; that his account of my calorimotor, and the object to which it had been applied, conveys an imperfect, or an erroneous impression.

The incorrectness of the assertion, that galvanic and electrical ignition "are entirely analogous," must be evident from the facts mentioned in my essay on the deflagrator, pages 274—5 of this Journal. The quotations subjoined will prove that this assertion is equally contradictory to the statements of some of the most skilful electricians.

Wilkinson's Galvanism, page 391.—"A powerful charge of electricity oxidates a considerable length of wire; galvanism destroys successive portions."

Singer's Electricity, page 40, third paragraph.—"If a fine iron wire of moderate length be made the medium of connexion between the extremities of the battery, it becomes ignited, and may be fused into balls; or if a platina wire is employed, it may be kept at a red, or even white heat, for a considerable length of time; which seems to prove, that some power is continually circulating through it; but *however powerful the battery, wires are never dispersed by it, as they are by the action of a charged surface.*"

Again page 338, paragraphs second and third. "Mechanical action is generally evident during the operation of common electricity, but such phenomena are rarely exhibited by the voltaic apparatus. Few are the instances in which the action of the electrical machine is unattended by the appearance of light, *yet it rarely exhibits any unequivocal effect of heat, but what may be considered as the consequence of its mechanical agency.* In the voltaic apparatus when no light is evolved, an elevation of temperature may be usually observed; and when, by its intense action, there is a copious evolution of light, *heat is produced in a superior degree to that which results from any other process of art.*"

That wires exploded by electricity are not proportionably heated, fully appears from experiments instituted by the celebrated Berthollet, one of the venerable coadjutors of the great Lavoisier, which I shall give in his own words, as translated into the Philosophical Magazine, vol. viii. page 80. "A wire of platina was submitted to shocks which were nearly strong enough to effect its combustion; and to be satisfied of this, a shock was excited by which a great part of the wire was melted and dispersed; afterwards the shocks employed were a little weaker, and immediately after each the wire was touched to judge of the temperature it had acquired: a heat was felt, which was dissipated in a few minutes, and which, at the utmost, was estimated to resemble that of the boiling point of water. If electricity liquefied metals and brought them into combustion by the heat it excites, the platina wire must, after a shock which differed but little from that which would have produced its dispersion and its combustion, have approached the degree of temperature which occasions its liquefaction: now this degree, which is the most elevated that can be obtained, would, according to the valuation, more or less accurate, of Wedgwood, be 32277 degrees of Fahrenheit.

"When the shock is sufficiently strong to destroy the aggregation of the platina wire, it begins by detaching moleculæ from its surface, which exhale like smoke; if it is strong enough to produce combustion, the remains of the wire appear to be torn into filaments.

"A thermoscope, blackened with ink, and placed in the stream of a strong electric spark, only experienced a dilatation which was nearly equal to one degree of Reaumur's thermometer; and this slight effect might depend on the oxidation of the iron of the ink: placed beside the current, it did not shew any dilatation, although the air was necessarily affected by the electric action: it was the same when it was placed in contact with a metallic conductor which received a stream less powerful than in the preceding experiments.*

"A cylinder of glass [illegible] with air, with an exciter at each of its extremities, [illegible] of which was fixed a tube, communicating with [illegible] cylinder filled with water, produced an impulse at each shock which raised the water more than a decimeter above its level; but its effect was instantaneous.

"These experiments seem to me to prove that electricity does not act on substances, and on their combinations, by an elevation of temperature, but by a dilatation which separates the moleculæ of bodies. The slight heat observed in the platina wire is only the effect of the compression produced by the moleculæ which first experience the electric action, or which experience it in a greater degree; it must therefore be compared to that excited by percussion or compression.

"If the dilatation was the effect of heat, that experienced by a gas in the experiment related above, would not have been instantaneous, it would only have experienced a progressive diminution by cooling, as when its expansion is owing to heat."

"An experiment of Dieman and his learned associates confirms this explanation: They caused a shock to pass through lead placed in a vessel filled with azote gas, which could not oxidate it; it was reduced into powder retaining all its metallic properties: If it had experienced a liquefaction similar to the action of heat, it would have cooled gradually, and would have congealed into one, or at least into several masses."

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* A small thermometer in the luminous current between two balls of wood, is raised thirty-two degreés.—Nairne.

It is observed, page 159 of the Review, " secondly, common electricity is also capable like galvanism of effecting chemical decomposition. To this fact too Dr. Hare seems to have thought it necessary to give a new explanation, and he supposes that the decomposition is produced in one case by ' divellent polar attractions exercised towards the atoms,' and in the other ' by mechanical concussion, or that process by which the particles of matter are dispersed, when a battery is discharged through them.' That the atoms which are combined to form the integrant molecules of a chemical compound can be shaken asunder by mechanical concussion, is so strange, and so entirely at variance with all we know respecting the power of chemical affinity, that we are surprised to find it even casually suggested by Dr. Hare, and are convinced this idea cannot retain his sanction."

It does nevertheless still retain my unqualified sanction; nor can I conceive that any person practically skilled in chemistry, would even casually suggest, that mechanical concussion cannot sunder atoms chemically combined. How can we otherwise explain the explosion of fulminating powders when crushed by a hammer.

Agreeably to the quotations made in my memoir, page 279 of this Journal, it appears that the atoms chemically combined in brass, in oxides of tin, or mercury, have been separated by electrical discharges; the shock produced by which, is the kind of mechanical concussion which I specified. Again I ask, are we to have one rationale for explaining the separation of oxygen from tin, or mercury, and another when it is separated from hydrogen by the same agent? It cannot be pretended that in these instances divellent attractions are displayed, for the particles are thrown apart from the wires which transmit the electricity instead of being attracted towards them.

In order to demonstrate that the chemical effects of galvanism, and those of mechanical electricity are " entirely analogous," he adduces, page 160, the decomposition of water as effected by the last mentioned agent, in the experiments of Wollaston, omitting a very striking trait of dissimilarity mentioned by the author of them. In his account of the experiments in question, the following important statement of that great chemist is not to be found. "*But still the resemblance is not complete, for in whatever way I have tried it, hydrogen and oxygen gas were given off at each wire, instead of their being formed separately, as in the case of the electrical pile.*" This is the very circumstance which justified my observation, that the disunion was effected by an explosion, not by divellent attractions.

The allegation that my own statements are contradictory, page 163, is predicated on the following passage, unaccompanied by the last sentence, which limits the meaning to the *chemical* effects of the electrical and galvanic fluids. "The opinion of Dr. Thompson,* that the fluid of the pile is in quantity greater, in intensity less, than that evolved by the machine, is very inconsistent with the experiments of the chemist abovementioned, who before he could effect the separation of the elements of water by mechanical electricity, was obliged to confine its emission to a point imperceptible to the naked eye. If already so intense, wherefore the necessity of a further concentration? Besides, were the distinction made by Dr. Thompson correct, the more concentrated fluid generated by a galvanic apparatus of a great many small pairs, ought to resemble that of the ordinary electricity; *but the opposite is the case. The ignition produced by a few large galvanic plates, where the intensity is of course low, is a result most analogous to the chemical effects of a common electrical battery.*"

I consider the fusion of wires as the chemical effect in which galvanism and electricity most resemble each other, because in this fusion, no divellent action of the poles is to be perceived, whether caused by one or the other. This being assumed, and the greatest quantity of wire having been fused, by a few large pairs, I conceive voltaic batteries of this kind to produce chemical effects the most analogous to those of an electrical battery.†

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* I ought to have said Mr. Nicholson.

† Having expressly stated, that the difference between the fluid given out by

The reviewer remarks, page 162, "Dr. Hare has indeed proved, and it is a new and interesting observation, that where the electric intensity is very low, as in his battery of two large galvanic pairs, the thinnest piece of charcoal intercepts the calorific agent, whatever it may be;" but to make this of any importance, as an argument in favour of his theory, he ought to have shown at the same time, that the "electricity was allowed to pass on." This he has not done; and in all probability it was not the fact.

My motives for constructing the calorimotor, are thus stated in my memoir:—"I had observed, that as the number of pairs in Volta's pile had been extended, and their size, and the energy of interposed agents lessened, the ratio of the electrical effects to those of heat had increased, till, in De Luc's column, they had become completely predominant; and on the other hand, when the pairs were made larger and fewer, as in Children's apparatus, the calorific influence had gained the ascendancy. I was therefore led to go further in this way, and to examine whether one pair of plates of enormous size, or what might be equivalent thereto, would not exhibit heat more purely, and demonstrate it, equally with the electric fluid, a primary product of galvanic combination." It was then my object to show, that as the phenomena produced by De Luc's column are apparently those of pure electricity, so in the calorimotor they would prove almost exclusively those of pure caloric. In order to demonstrate

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galvanic apparatus and that yielded by an electrical machine, became evanescent in very numerous series of very small pairs; the reviewer treats this as a case adduced by myself, in which the galvanic and electric fluids are most alike. The object of my hypothesis was to explain why the fluid of the voltaic pile when evolved so as to *display its characteristic potency, in which case only I call it the galvanic fluid*, differs from that which *is admitted* to be electricity (whether evolved by an electric column, a machine, by a pile excited by water only, or by one of De Luc's dissections) in its chemical power, without producing a correspondent increase of genuine electrical phenomena. That electricity, to all appearance pure, is evolved by the pile in the abovementioned cases, is undeniable. But in this state, it is not the galvanic fluid, any more than the caloric evolved by a wire in the forceps of a calorimotor, or in the heated arch between the poles of Sir H. Davy's battery. That fluid only can be called the galvanic fluid, which is peculiar to galvanism, and which as it produces the effects of both heat and electricity, cannot be supposed identical with either alone.

that no palpable quantity of the former could be extricated, I placed in the circuit a substance which would allow the fluid to pass, if it were electricity, but not, if it were caloric. The reviewer falls into the very "strange" mistake of supposing, that if the instrument realized my expectation, and yielded caloric "*purely*," *electricity* ought to "have passed on." From his placing the words "electricity was allowed to pass on" under marks of quotation, it is evident that he has confounded [illegible] the calorimotor, where by the premises cal[illegible] co[illegible] pear, with another, where a [illegible] n, both fluids were suppo[illegible] tion, that either might be detecte[illegible]

My men[illegible] words, "I have for some time [illegible] uid of the pile is a com[illegible]" I used the word *electro*[illegible] nd; and stated that with[illegible] mpossible to explain their passage to the positive pole, in piles of thousands of pairs, through air and moisture; the one so well calculated to conduct away electricity, the other so favourable to the radiation of heat. Yet the reviewer observes: "In the theory of Volta, substitute caloric for electricity, and you have the theory of Dr. Hare;" and appeals solely to the properties of heat to disprove an hypothesis, founded on the idea of an indispensable union between caloric and electricity. He says, page 164, "Volta supposed the electricity extricated by his apparatus to be the consequence of an electromotive power in the plates. Dr. Hare, believing caloric an "original and collateral product" of the pile, has had recourse to a calorimotive power to account for its production. In the theory of Volta, substitute caloric for electricity, and you have the implied theory of Dr. Hare. Let this be attempted; treat of the resinous and vitreous, or, if you please, of the positive and negative *caloric*; consider the mode of action of the interspersed humid bodies, pursue the analogy into its details; reconcile these views with the known properties of heat, and you will find that you have undertaken a difficult, perhaps an impossible task."

These comments are not more inconsistent with the passages above quoted, than with another (in my memoir) which I subjoin. "Indeed, when the forty plates were successively associated in pairs of copper and zinc, though suspended in a fluid held in a common recipient without partitions; there was considerable intensity of galvanic action. This shows, that *independently of any power of conducting electricity*, there is some movement in the solvent fluid, which tends to carry forward the galvanic principle from the copper to the zinc end of the series. I infer that *electro-caloric* is communicated in this case *by circulation*, and that in non elastic fluids, the same difficulty exists, as to its retrocession from the positive to the negative end of the series, as is found in the downward passage of caloric through them." Instead of attributing the progress of the galvanic principle from the zinc to the copper through the fluid interposed, as effected, agreeably to Volta's opinion, by the conducting process; I ascribed it to a circulation, founded on the well known fact, that fluids unless by a circulation of their particles, are almost absolute non conductors of caloric. The bulb of an air thermometer, covered by water one third of an inch deep, is not heated by the inflammation of ether on the surface of the water. Yet, at the same time, slightly touching the bulb with the finger, will give warmth enough, to cause the fluid in the stem to move. Water may be made to boil on the top of a tube, without being warmed a few inches below. When a lamp is applied at bottom, the particles growing warmer and lighter, rise and give place to the colder. Heat can therefore hardly traverse water, unless its particles are by some means forced to circulate. The diminution of specific gravity consequent to expansion, is the cause of circulation in the instance above adduced: but the passage of "*electro*-caloric" in the voltaic apparatus, is attributed by me to the known attraction of the galvanic plates, (however arising, whether from their electrical state, transferred attraction,* or any other cause) which obliges particles surcharged with this fluid to go from the zinc to the copper. Volta, on the other hand, considers, that the fluids act by conducting the electricity: a rationale completely at war with my hypothesis, and contradicted by my late experiments, where, in a series of eighty pairs, partitions in the troughs, are shown to be unnecessary. In the absence of insulating partitions, the conducting process would carry the fluid one way, just as fast as in the other

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* According to Mr. Donovan's theory.

I will corr[illegible] than I proposed. Speaking of the intens[illegible] carbon in the voltaic circuit, the reviewer, in or[illegible] to [illegible] pression I had given, says "we have no[illegible] met [illegible] experiments to show that it does not di[illegible] his [illegible] ristic with common electricity."

The intense ignition of [illegible] n is noticed as an effect of galvanism in every galvanic treatise, while in books on electricity its ignition by this principle is not mentioned. It is then a lame objection to the idea that mechanical electricity does not ignite it, to say "*we have not heard that it has not this habitude.*" I have myself subjected it to electrical sparks without seeing any marks of ignition. We are informed in a passage cited by the Reviewer, that Van Marum while testing the calorific power of an electrical machine, used charcoal. It can scarcely be doubted, that if this substance were liable to intense ignition by mechanical electricity, he would have detected and mentioned this characteristic. Having the bulb of his thermometer already within a piece of charcoal, and desirous of heating the instrument to the utmost, by placing it in the circuit between his conductors, he takes it out of the coal, and suspends it "loose." According to Wilkinson, vol. ii., page 330, "Some persons have supposed galvanism to differ from electricity, because the conducting powers of charcoal have been found to be greater for the former, than for the latter principle. This is however, far from being the case. Charcoal preserves the same rank in the order of conductors, both for electricity and for galvanism. That it is found superior to metals, in the deflagration of the latter substances by galvanic processes, does not depend upon its conducting power; but upon its aiding principle of combustion." It must be evident from this, that charcoal has been found to aid by its combustion when exposed in the galvanic circuit, and that it had not been found to do so, when exposed to a current of electricity.

As my theory is founded upon the idea that there is an affinity between heat and electricity; when the last mentioned principle is made to pass through bodies containing the former, in such a state of intensity as to enter their pores, it is consistent that it should on coming out of them, bring with it some caloric. As in the case of mechanical electricity, heat is not generated simultaneously with that fluid, it can yield no more than it may have obtained in its passage through bodies, which may have subsequently conducted it. Hence its heating influence is feeble. The thermometer in Van Marum's experiment rose only from twenty to forty degrees, whereas quartz and magnesia, which being infusible by a furnace, require at least 25,000, have been melted when exposed in like manner to a galvanic current.

In alleging (page 167) that the maximum of igniting power had been surpassed by Mr. Wetherill's large pair, an impression is conveyed one way, or the other, very erroneous. In quantity, the maximum had not been attained; and the maximum of intensity, must be looked for in a very minute pair, like Wollaston's. I never pretended that my calorimotor differed, unless in its form, size, and the nature of its powers, from the original galvanic pair, or that there is not a principle common to galvanic pairs under every form. The Reviewer, instead of deriving the principle of its action from the original pair, refers it to Wollaston's abridgment of that pair, regardless of my having ascertained, that altering the size of the apparatus varies the nature, as well as extent of its powers. The opposite alterations made in it by Wollaston and myself, cannot be identified. Where claims are founded not on things but the changes made in them, they must cease when the changes are reversed.

Alluding to my account of the calorimotor, the Reviewer observes (page 166), " It is impossible to read this description without perceiving, that it is essentially the same instrument as the elementary battery." Yet after reading this description, Dr. Thomson in his notice of my theory and apparatus, in his Annals of Philosophy, speaks of the latter as a "new modification of the galvanic battery."

Since writing the above, I have endeavoured in every mode which I could devise, to ignite charcoal by electricity. Exposed to the discharge of a powerful battery in pieces tapering to a point, in a glass tube, in thin strips, and in powder, by means of the glass usually employed for inflaming ether, it was either uninfluenced, or merely dispersed, without the smallest symptom of ignition, or even of increased warmth. Yet fulminating mercury was flashed by the discharge, under the same circumstances as those in which the powdered charcoal had been subjected to it. The result therefore was such as might be expected from a "*mechanical concussion.*" Pointed wires were covered with spermaceti, and exposed to a current from a fine plate machine of thirty-two inches diameter; yet no sign of fusion appeared. Nor was a differential thermometer filled with ether, according to Dr. Howard's sagacious plan, affected sensibly; though the warmth of a finger applied to the bulb, caused the fluid in the stem to move nearly a foot.

I mentioned in the memoir, page 275 of this Journal, that when a knob of lead suspended by a filament to one of the poles of my deflagrator, was made to touch the other pole of the same instrument, the knob was fused, the filament uninjured. I find the reverse is the case, when a knob suspended by a filament, is made the medium of discharging an electrical battery. The filament is destroyed, the knob remains unchanged. It must be evident, therefore, that galvanic and electrical ignition, far from being "entirely analogous," are extremely discordant in their characteristics.

It is also mentioned in the memoir, that a piece of silvered paper two inches square, proved inadequate to discharge my galvanic apparatus of coils, yet at a distance seventy times greater a strip of the same paper one third of an inch wide, and twenty inches long, caused an instantaneous discharge of the electrical battery.

Art. VIII. *Remarks on the Reputed Sanative and Prophylactic Virtues of Scutellaria Lateriflora or Scull-Cap.* By William P. C. Barton, M. D. Surgeon, U. S. N.

"Id est impossibile, quod fieri non potest."

It is now more than twelve years since a marvellous story was promulgated to the world, concerning the sanative and prophylactic virtues of scutellaria lateriflora or scull-cap, in relation to hydrophobia. The story, divested of its garnitures, and brought to a tangible shape for medical scrutiny, avers that an inert weed, possesses peculiar virtues, and produces extraordinary effects; that the *decoction* of a plant wholly inactive, is endued with palpable activity; that a tea, wanting sapidity or any other sensible property indicative of medicinable power—incapable of affecting, by any perceptible operation, the system, or any part of it—neither enhancing the vigor, nor depreciating the energy of the animal functions—that this decoction may be gulphed with impunity to any extent, that the stomach may receive simple water, not only without rejection but without the disturbance of its functions, which cannot act in any way on that organ except by its mechanical property of weight, and the distention it may produce by mere bulk; which may be thus taken as long as one might choose to gorge himself with it, without the least evidence of medicinal effect—that this feeble drink, prevents the accession of hydrophobia after the bite of a rabid animal, and infallibly cures this direful malady! Thus have we an herb possessing no powers of any kind, producing no effects more than a decoction of paper or straw, being absurdly called a medicine, and praised as a remedy for a malady always disastrous and generally fatal! The wonderful energy of this decoction, is related to be equally manifest, either as a prophylactic or cure, whether man or brutes are the subject of its operation. It is to consider with seriousness and without bias, its effects upon man, that I have entered on these remarks; and it may be proper to state what opinions have led me to a decision of the expediency of attaching an adventitious, but, I trust, a temporary importance to the subject, by raising to the dignity of a professional scrutiny, a story, which by some may be thought unworthy of that much notice, seeing that, to the reflection of any one who will earnestly weigh its worth, it carries its own contradiction with it.

The public prints had for a long time past teemed with commendatory accounts of the alexipharmic and curative efficacy of the scutellaria lateriflora.[*] And, had they been confined to newspapers alone, and in them been simply supported by their editors or correspondents beyond the pale of medicine, any serious investigation of their fallacy would have been unnecessary. But unhappily, and very unaccountably, notwithstanding the palpable evidences of fable which many of the accounts present, and the opposition to the dictates of common sense by which they are characterized, medical men of reputed respectability have not been wanting, ready and willing to sustain this unreal tale. It is true the number of those who have supported the dangerous delusion is small; but still it is sufficient to give the idea of a division of medical sentiment on the subject; and, though this idea may readily be proved unfounded, it leaves the unfortunate but natural effect of every collision of opinion on professional points,—that the public confidence must be weakened on every point in which their oral or written testimony is delivered, thus sapping the foundation of legitimate pathological report.

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[*] It has happened unluckily, that the scull-cap has found a champion of no common character. One not educated to physic nor versed in its principles, and consequently unfit, as I shall take leave to assert, though I do so without the slightest derogation from his talents or acquirements, to form a correct judgment upon a point involving professional knowledge and scrutiny; yet this champion is one whose literary attainments, whose facility of writing, whose unparalleled enthusiasm in what he religiously believes a just cause for the benefit of humanity, and, above all, whose opportunities of giving publicity to his opinions are daily afforded and readily put in requisition to second the efforts of his pen, or promulgate the accumulations of his indefatigable industry, and what I fear too often happens, the unweighed testimony of his correspondents, all conspire to present the subject in no very assailable shape before persons uneducated in physic or uninformed on the general principles of its practice. I should, however, do injustice to my own impressions of that gentleman's character, and withhold from him that meed which I conscientiously believe him to deserve, did I refrain from observing that I do believe genuine philanthropy, seconded by an honest satisfaction of his own mind of the truth of the opinions and accounts he circulates, alone actuate his conduct; and under these impressions it is a matter of unmixed regret, that his time, his talents, and praiseworthy zeal, are wasted on a subject unworthy of either. That his writings on this subject have done much mischief I can have no hesitation in believing; and the extent of this mischief is augmented, and its intensity enhanced by the evidences of absolute personal conviction, which manifestly accompany all his remarks on the point. Though I do not flatter myself, therefore, to be able to disseminate these remarks through the medium of a scientific journal as extensively among all ranks of people as the notions they are designed to counteract have, favoured by the means, undoubtedly reached, still I hope the truth will eventually work its way even to their minds.

It is deeply to be regretted for the honour of our profession, that any American physicians should so far have forgotten the respect they owed themselves, as well as the dignity of their vocation, as to have fallen in and glided with the current of popular commendation, lavished, as is mostly the case, on a quack article of no power answering the indications of the disorder it is recommended to cure, and prevent: but not, as is often the fact, on an active or poisonous drug, which, in better hands, and under the discerning eye of an intelligent practitioner, might be advantageously used. This conduct is to be the more reprehended, since its injurious consequences are not confined to the drug in question; but, by multiplying the causes of just animadversion on the profession, is calculated to weaken its interests, and depreciate its respectability.

It is surprising, after the extravagant stories of the curative and antihydrophobic powers of so many nostrums had vanished in their own insignificance, that any physicians would again have afforded the sanction of their professional character to foist into notice, and mischievously vamp up for this purpose, another useless and inert drug. Yet, though wholly useless for the purpose it has been recommended to accomplish, and absolutely inert, as far as regards any sensible or operative effect on the system, it has not been employed without a degree of danger commensurate with the credulity which has caused its exhibition: for, by a blind reliance on its supposed virtues, the only means which ever have had power over the disease, have been neglected until it was too late for their exhibition.

Much mischief has been done by the wide circulation of the unfounded reputation of this plant. An extraordinary pamphlet by Dr. Spalding, has contributed, in no small degree, to extend the accounts; and, coming before the public in that pamphlet, as they did, under the auspices of a highly respectable physician, a more ready credence was given to them.

Under this view of the subject, it seems proper that no more time should be lost, in exposing the dangerous effects with which a belief in the powers of scull-cap is fraught.

The views which it appears to me fit to take on this subject, will be set forth in the following assumption of facts and principles, which I venture to believe will not be presumed questionable; and which, if they should be thought problematical, admit of ready and established proof, grounded on the principles of physic, the facts of medical records, and the ratiocination of philosophy.

First, That the disease of hydrophobia is one of strong and, generally, irresistible force; and when it has appeared, runs its course in a rapid and appalling succession of symptoms, which leaves no time for the arresting operation of tardy constitutional remedies, if even any there be endued with sufficient power to produce such an effect; that it consists, for the most part, after its first visible sally, of a single exacerbation, marked by a peculiar and invariable character, and not easily forgotten when once seen, nor ever after likely to be confounded with any other malady.

Secondly, That, such being the energetic and disastrous nature of the disease, it would be unphilosophical to expect sanative effects from any other means than those possessing a degree of energy, activity and promptness of operation, commensurate with the force of the symptoms, or capable of removing the prominent features of distress, or the cause of them.

Thirdly, That therefore, if either theoretically, or, in pursuance of the established principles which guide physicians in their practice in difficult and dangerous cases, it be deemed expedient to try new remedies for the disease in question: they should be taken from among the powerful articles of the materia medica, and not from a class of slightly stimulating or inert herbs. And, that from among the powerful remedies, one should be selected which is not, either by the quantity of liquid in which, as a vehicle it must be conveyed into the system, or by any other property, incompatible with the known difficulty of swallowing which produces the dread of fluids so generally prevalent in hydrophobia. For it is inconceivable that a person labouring under this inability to swallow, should be capable of gulphing large quantities of liquid physic.

Fourthly, That therefore, if we even grant that there is a remedy possessing power adequate to the force of the disease: yet, if it be requisite to administer that remedy in such a manner that it must necessarily aggravate the most distressing symptoms, it is wholly inadmissible; and, for the cure of that disease, as absolutely useless, as though it were not possessed of any power. And further, that if this remedy, which is supposable to possess a power equal to the force of the disease, were capable of being introduced into the system, the barrier opposed by the difficulty of swallowing, apart: yet even then, if it require to be used a long time to produce a remediate effect, it is of no value in hydrophobia, which moves with disastrous celerity from its first pathognomonic sign of existence, to its termination in dissolution. Acute diseases cannot be cured by chronic remedies, however powerful they may be: the speed of such disorders not allowing time for the tardy operation necessary to give effect to medicines thus chronically administered. Hence a disease which attains its acme in a few hours, or a few days at farthest, cannot possibly be cured by a remedy, supposing it powerful, which requires a continued exhibition for twenty or forty days.

Fifthly, That it is contrary to reason, to experience, and to the principles of the practice of medicine, or the laws of therapeutics, to expect sudden and violent effects on the system, or any part of it, from either the internal or external administration, or both simultaneously, of any inert drug or herb.

Sixthly, That it is in opposition to every known fact of a similar nature, to suppose, that a medicine can be active, or a drug effective or remediate, which does not produce in some definite dose, or different limited doses, repeated at specific times, some precise, visible, or alterative effect on the system. Every thing, either of a vegetable or animal nature, which can be taken into the stomach to an unlimited but reasonable extent, and which does not, in consequence, develop its effects by a sensible or conceivable operation on the system, or some part of it, being of the nature of mere aliment. And such things as are merely alimentary cannot be medicinable, in the genuine acceptation of that term.

Seventh, That every article of the materia medica, with which we have been hitherto acquainted, if active, evinces its activity, either in substance, or decoction, or infusion, or tincture. That an article possessing activity in a fluid state, or in a state of solution in a watery menstruum, must discover, if properly exhibited, if not a like power, at least a resembling activity, in its solid form. And that articles which are active, are generally most certainly so in solidity, or in substance. And this activity is commonly independent of mere bulk, very small quantities being generally sufficient to produce their characteristic effects.

Eighthly, That it is known to medical botanists, that all the articles of the vegetable materia medica, which possess simply a gentle aromatic warmth, and which are wholly destitute of power, or any conspicuous operation on the system, are such as belong to the natural families *Labiatæ* and *Scrophulariæ* and natural order *Verticillatæ*.

Ninthly, That it is a fact of such common occurrence, for different species of the same genus, and the different genera of the same natural family, to be endued with a similarity of virtue, and produce a resembling effect on the system:

that it is considered by some tantamount to a law in the ascertainment of power or activity in plants. Hence, if the natural family to which a new discovered plant or newly reputed medicinal vegetable, be fraught with genera of medical power: the alliance of the plant in question to such genera, and its place in the natural family by which they are grouped, afford legitimate grounds for a reliance on any reputed remediate powers; or yield reasonable expectations of finding a similar activity to that which characterises the vicinal genera. The reverse is true. Where the natural family and genus are inert, a species will be so.

Tenthly, That every medicinal vegetable discovers its claim to this title, either in its sapor, its odour, its aroma, its pungency, its power of producing cutaneous irritation, or superficial inflammation when topically applied to the system, its volatility, or by some other visible or sensible effect; and that where some one of these attributes is not conspicuous, or at least discoverable on scrutiny, the vegetable must be inert.

Having assumed these data as premises, on the full assurance of their validity, it now becomes proper to investigate, by them, the claims of scull-cap to be ranked as a medicine of herculean power, thus testing its virtues and the principle on which it has been used. In doing so my object is to show, that nothing can be more unfounded than these claims; for, tested by correct reasoning, authentic rules, and stedfast facts, scull-cap is *in every thing found wanting!*

I shall begin my scrutiny of the powers of the herb, and the testimony in its favour, in the order of the principles just assumed as irrefragable.

And, *first*, it appears to me sufficiently evident, that if the nature of hydrophobia be what it is represented in article first, most, if not all of the cases reported to be such by the informal reporters who declare them to have been cured by scull-cap, were not cases of hydrophobia; and, if any thing more than imaginary diseases, have been cases of some of the protean forms of hysteria, or some other nervous affection. This appears the more likely, because the symptom *hydrophobia* occurs in genuine hysteria, and has also occurred in other nervous diseases, which were not produced by the bite of a rabid animal.

Secondly, That if sanative effects in this disease can only be rationally or philosophically expected from articles posessed of energy, activity, and promptness of operation: it is absurd to look for them in scull-cap, for reasons stated in conclusion number six and seven.

Thirdly, That if it has been correctly stated, that new remedies for this disease must be derived from among the powerful articles of the materia medica, and not from among a class of inert herbs, it is vain to try the scull-cap, or expect advantage from it, since it belongs to the latter class, as is evident in what is stated in conclusion seven; and its administration is further incompatible, for the most part, with a common symptom of the disease, viz. the difficulty of swallowing. Hence it could not, even supposing that it be adequate to the cure of the disease, be administered to the extent, and in the manner stated, because the hydrophobia would form an effectual barrier against this means of introducing it into the system: for the spasms, constricting the pharynx and closing the jaws upon the associative actions of the mind with the cause of fear, would seem quite sufficient to show how vain must be the dependence on the sanate virtues of such a medicine. Indeed, nothing can be expected from any remedy requisite to be introduced into the system through the medium of large quantities of this or any other fluid, the dread of which so frequently, though not indeed always, characterizes the disease. Can any thing be proposed less rational than such a means of relief? any thing less feasible of performance, than forcing down the throat, already closed by spasm, a fluid medicine, the sight of which must aggravate, and not remove, the existing symptom of distress?

As this terrific symptom seems to owe its existence to spasm of the pharynx, and temporary, perhaps partial paralysis of the muscles of the throat and deglutition, produced primarily by the fear of suffocation which the attempt to swallow produces, and continued subsequently, by wrong or inverted associative actions: there is absolutely no *opportunity*, at least in nine tenths of the cases which occur, of exhibiting the *scull-cap* or *any liquid physic.* Even solids are abhorred, from the pain and difficulty of swallowing. And, if it be said that in one case out of ten there do occur intervals, when the muscles of deglutition are again, as in health, subjected to the associative power of volition; and that such are the precise moments for administering this medicine, I would answer: if such practicability of swallowing fluids and solids does occur in this disease, (and, from experience I know it sometimes does,) the favourable moments should not be lost, by deluging the stomach with a useless quantity of inert fluid, which can in no wise operate, except by mechanical distention; but in seriously attacking the disorder with such efficacious remedies as shall, by their *known* promptitude and power of action on the system, lead to a rational expectation of lessening or removing the prominent symptoms; as well as in reducing the system simultaneously, by free and copious blood-letting, below the point of spasmodic action or muscular resistance.

Fourthly, That, admitting, *argumenti causa*, the medicinable efficacy of scull-cap, its use would be difficult, if not impossible, in this disease, as has just been shown: since it must be taken, it is said, in a *liquid* form, and hence painfully aggravate a most disastrous symptom. And, if, by a further extension of this supposition, it be granted that a potency exists in scutellaria commensurate with the force of the disease, and if we also admit the feasibility of conveying it into the system: it is still absurd to expect relief or cure from it, since it requires the lapse of a greater length of time than real hydrophobia from the bite of a rabid animal, was ever known to exist, before its efficacy could be visible. As a prophylactic, it is said it must be given for forty days; hence, as a cure, it is reasonable to suppose even a longer time would be necessary, because more of its power and peculiar efficacy would be needed to cure so violent a dis-case, than simply to guard the system against one which did not exist. This cannot be doubted, for in the one case vigorous and morbid derangement of the animal functions is to be contended with and overcome, and that too, very speedily; while in the other, the preservation of these actions of the healthy system is all that is necessary to be done.

Fifthly, That if what is stated in article five, be truth, viz. that sudden and violent effects on the system, or any part of it, cannot be expected from either the internal or external administration, or both simultaneously, of any inert drug: that, in consequence, such effects will not follow the exhibition in either or both ways, of scull-cap, which is fully entitled to that character.

It is not capable of making any kind of impression on the system, either diseased by hydrophobia or otherwise disordered, or, in health. And if even it were possessed of the power of doing so, it could not have cured hydrophobia, as stated, by large quantities of the decoction being poured into the stomach: since, had the disease existed, this would have been impracticable—and if it did not in such cases really exist, the scull-cap could not have cured it.

Sixthly, Scutellaria lateriflora is an inert vegetable in whatever form it be prepared, or in whatever manner administered. It has no effect directly or indirectly, on the general system, or any part of it, evidenced at least by those marks of operation which physicians and pathologists have universally agreed on, as unequivocal evidences of medicinable power. It is not pretended by those who recommend it, to be limited by small, and scarcely by any definite dose—nor is it restricted to any specific times of repetition, even in the discretionary dose recommended. By way of experiment I have drank it to the extent of a pint within half an hour, with impunity. Hence, though I would not assert, nor do I suppose, that it is nutritive, yet it must be considered rather as an aliment than a medicine.

Seventhly, Scull-cap evinces no activity, or medicinal power, either in substance, or decoction, or infusion, or tincture!!! Surely if it is endued with the great energy it is reported to possess in *decoction*, that is, diffused in a watery menstruum: it is consistent with facts in similar cases, with experience, and with reason—to expect it would show a similar, perhaps more intense activity, when given in a solid form. But so far is this from being true, that, in a solid form I have not only tried it, and found it unavailing in hydrophobia; but have no hesitation in pledging myself to administer any quantity, not offensive from mere bulk, to any number of persons, willing to make the experiment, without the slightest perceptible effect discernible by the senses, or even rationally conceivable by the understanding.

Eighthly, The genus Scutellaria belongs to a very natural assemblage of plants, grouped in the Natural Families, with the following genera. Ajuja, Amythestea, Brachystemum, Betonica, Ballota, Clinopodium, Collinsonia, Cunila, Dracocephalum, Glechoma, Germanea, Hyptis, Horminum, Hyssopus, Isanthus, Lamium, Lavandula, Leonurus, Lycopus, Marrubium, Melissa, Mellilitis, Mentha, Monarda, Nepeta, Ocymum, Origanum, Prasium, Phryma, Perilla, Prunella, Rosmarius, Salvia, Satureija, Sideritis, Scutellaria, Stachys, Teucrium, Thrymba, Thymus, Trichostema, Ziziphora.

Of these genera six are used freely in diet—viz. Marjoram, Thyme, Sage, Lavender, Sweet Basil, Mint—and of those possessing any medicinal virtues, the genus Mentha, afford the most powerful product, in the oil of mint.—The properties of Melissa or Balm, Cunila or Pennyroyal and Dittany, Marrubium or Horehound, Nepeta or Catmint, are well known. Rosemary has been lately commended as an emmenagogue, but all the other genera are wholly inert.

Ninthly, Tested by the facts of common notoriety to botanists already stated in detail, scull-cap would not be expected to possess any activity or medicinal virtues, beyond balm, sage, dittany, horehound, catmint, pennyroyal, motherwort, summer savory, marjoram, &c. I have shown that the whole tribe with which it is allied by nature, possess in common, either no medicinal power whatever, that is, are inert; or, if any, are endued with the slight degree of warmth and stimulation, which the common bitter family herbs (all belonging to this tribe) are possessed of. Rosemary is the most powerful of the whole family! allow scull-cap equal virtues with it—can rosemary cure or prevent hydrophobia? But further, the genus scutellaria is remarkably inert; the most powerful species is S. hyssopifolia;* and this is simply very bitter. S. galericulata is said by Schœpf, to have cured intermittents,† and he intimates that S. hyssopifolia, and lateriflora, may do the same. I am intimately acquainted with every American species of this genus, and can with confidence declare, that the most powerful is the S. hyssopifolia, which perhaps scarcely equals in strength of bitterness, the flowers of camomile.—Is camomile adequate to the cure or prevention of hydrophobia?

Tenthly, Scutellaria lateriflora is endued with no sensible properties. It is devoid of taste or smell, except what it possesses in common with every inert plant hitherto discovered. From the universality with which such odour and taste as it does possess, pervade the inert herbs or plants, they are, contradistinctively to any sapidity or odour evidencing medicinable power, or possessing any energetic character, termed herbaceous. The herbaceous smell and taste must be recognized by every one, when it is mentioned, that they are possessed by all the grasses, the common weeds, and way-side vegetables. Scull-cap has no pungency nor aromatic principle. It does not vesicate nor produce a rubefacient effect when applied to the skin; neither does it create heat, itching, pain, nor increased salivary discharge upon mastication. It produces no sensation in the stomach, nor head, nor brain, nor throat, nor lungs, nor liver, nor heart, nor bowels. It consequently neither affects respiration, digestion, nor any other vital function in the smallest degree. It does not stimulate the mucous membranes. It neither affects the fauces, like seneka, in passing them, nor produces sneezing by being snuffed up the nose in considerable quantities, like tobacco and other sternutatories. It has no volatile part. The evaporation of a saturated decoction, discovers no exhalation but an herbaceous one; and the residual extract, after exsiccation by slowly suffering the watery particles to be carried off near a stove-heat, is analogous in all respects, to a similar product of common grass (*poa viridis.*) As, therefore, none of the attributes indicative of activity, or medicinal power, or poisonous effect, are visible or discoverable in this plant, I am justified in concluding that the *far famed scull-cap, the vaunted preventive and cure of hydrophobia and* "*tetanus*,"* is wholly and essentially inert, and consequently medicinally worthless. This assertion is not gratuitous: it is a logical inference grounded in reason, and sustained by experiment and the irresistible nature of the preceding facts. That no one interested in the subject may feel his doubts unsatisfied, or his mind uninformed on this point, I hereby declare myself willing and ready, to present the scull-cap to any one disposed to go to the trouble of an experiment, that he may reiterate the trials I have repeatedly made to full and entire satisfaction.

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* For a figure and history of this plant, I beg leave to refer to my Flora of North America, Table 2.

† In the "Materia Medica Americana potissimum Regni vegetabilis," of Schœpf, published in 1787, the first notice is to be found of the medicinal virtues of the genus scutellaria. He says of S. galericulata, that it is sub-odorous and bitter, an abstergent and tonic, and useful in tertian fevers; that S. lateriflora, integrifolia and hyssopifolia possess the same virtues, "viribus cum hac conveniunt."

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* See late publications on this subject in the New-York Evening Post, particularly one in that paper of the 16th of January, 1821. If any thing more were necessary to present the scull-cap in its true light, thereby discovering its inefficacy, and the empirical views by which it is commended, such is afforded by this case. Even the names of the two highly respectable physicians mentioned in it, will not have the effect of persuading any reflecting physician, particularly if he have ever seen or treated a case of tetanus, that aught but opium, wine, and brandy, had any thing to do in the recovery of this boy. It is stated in the report, that he took "eight grains of opium, from day-light till two o'clock P. M. and a few spoonfulls of wine;" and besides this he took the opium "twice,"* and then again "once," and about half a pint of brandy—all between the day-light of the eleventh inst. and the second morning after that time. Thus had he taken at least nine and an half grains of opium, the most powerful antispasmodic of the materia medica, a few teaspoons full of wine, and half a pint of brandy. And he had taken a wine glass full unremittingly every fifteen or twenty minutes of the decoction of scull-cap, made by pouring a pint of boiling water on an ounce of the dried herb, during all the time elapsing from two o'clock on the eleventh, after having, between that hour and the preceding day-light, taken *eight grains* of solid opium. It is seriously asked whether the inert scull-cap or the opium, brandy, and wine, cured this disease?

* Say half a grain.

Having now, by reasons, facts, and arguments, shown that scull-cap is not only an inert plant, but that its medicinal immerit is remarkable; and therefore, from the nature of things, incapable of achieving the powerful cures declared to follow its administration: it remains for me to offer a few observations on the nature of the testimony in favour of the scull-cap; and then to close the detail, by a history of a case of real hydrophobia, and another, in which the imagination of the patient was fast leading him into a full development of all the direful symptoms of this malady. In both cases I prescribed the scull-cap; with what views and success will be seen in the sequel.

The accounts of the prophylactic and curative virtues of scull-cap, are not only improbable but marvellous in themselves; and carry with them, strong evidence of inconsistency, ignorance and empiricism. That they are improbable is evident, because they are incompatible with reason, experience, and the known laws of the animal system, the force of disease, and the laws of therapeutics; they are marvellous, because unaccountable by any rules of philosophizing, or by the inductions of reason; they are inconsistent, because contradictory and irreconcileable with truth and with each other; and evidently charlatanical, because they support the idea that a violent disease has been and can be cured, by a mean inadequate to such an effect, and so feeble, that no limits are set to the dose, nor any specific restriction as to the times of repetition of the doses. They are empirical for this additional reason; that though some few doctors are stated by Dr. Spalding in his pamphlet, to have reposed great confidence in the prophylactic and curative powers of the plant in hydrophobia, they forfeited, by the very circumstance of their making a secret of the supposed remedy, any claim to be longer considered candid and regular practitioners. The mass of those, however, who practised with this plant, are not only very illiterate men, but they did not belong to the profession of medicine, being weavers, &c. &c. I will not conceal, however, the fact, that some respectable physicians, as Dr. Thatcher, Dr. Stephen H. Williams, and one or two more, recently, have declared their confidence in the reputed power of the plant. But in doing so, there cannot be a doubt that they have reprehensibly suffered themselves to be misled by the importuning publications on the subject, with which our newspapers have been unfortunately replete. If however, those gentlemen will make experiments on themselves or others, in health or disease, with the plant variously prepared and administered, I cannot for one moment doubt that they will see their delusion. Dr. Spalding's conduct deserves a more severe notice. His pamphlet, to speak in the mildest language which the case admits of, is the most empirical production which ever came from the pen of a regularly educated physician; it is fraught with mischief, full of absurdities, and replete with extravagancies entirely inconsistent with the reflection of such a physician as we have hitherto considered him. In a word, it reflects serious injury on his professional character, and is disrespectful towards the profession generally. Yet, I ought to add, this is said on the mere merits of the pamphlet, connected with the great respectability of its author, and independent of his categorical disavowal *subsequent* to its publication, of any belief in the truth of the statements to which he had given in it an unnecessary publicity. This does in no degree lessen, but actually aggravates his reprehensibility in publishing the pamphlet; yet justice demands that his disbelief should be registered, so as to counteract the danger his publication may produce. I have therefore much pleasure in declaring that Dr. Spalding is, by his own request, striken from the list of believers in the preventive or curative powers of scull-cap, and doubt not that his example will soon be followed, by others who remain on it.

The testimony in favour of the preventive power of scull-cap, is for the most part negative.—It consists of an enumeration of numbers of persons and animals bitten by rabid dogs, hogs, &c. who had taken the decoction and did *not* become mad. It is also suspicious—because it chiefly emanates from the vulgar or illiterate, who are prone to exaggeration in similar cases, from the mere love of the marvellous—and when from intelligent persons, it is, with few exceptions, from such as are not of the medical profession, and consequently not fully adequate to a just conception of the subject. For I believe it will not be denied by any one acquainted with the history of physic, that mere practical medicine has never been essentially improved, except by physicians.

The testimony is further incredible, because of the immense number of persons and cattle said to be bitten by rabid animals, prevented from getting mad, and cured of hydrophobia—thus evidencing a degree of exaggeration, which casts an air of suspicion on the whole testimony. Misrepresentation or misapprehension in these accounts, are discoverable in some circumstances which admit of absolute refutation, by experiment;* and if this has occurred on points which admit of demonstrable contradiction, the inference is natural and consonant with reason, that similar misrepresentation or misapprehension exists, in such points of the testimony, as are not tangible—which by their very nature are obscure, and beyond the reach of scrutiny or detection, or subversion.

The reports on this subject are not to be relied on, so far as regards the effects ascribed to scutellaria lateriflora, because there is much reason to believe one half of the cases in which this plant has been stated to have been given, were not treated by it, but by some other resembling plant. I infer this from the fact, that within the last three years, numerous applications have been made to me for information whether certain plants brought for my inspection, were the "*genuine scull-cap*."—In but three or four instances has the scutellaria lateriflora been shown me, and perhaps in two or three more, other species of scutellaria have been presented. Many of these persons have informed me, that the plants brought, were those collected and used by different persons to prevent and cure hydrophobia, under the full belief that they were the *true scull-cap*. And several strong cases were positively stated to have yielded to the scull-cap brought as a specimen, and which was *Verbena hastata*, a plant well known to botanists, as a common inert weed. Mimulus ringens has been sent to me from New-York, by a very conspicuous advocate for the power of scutellaria lateriflora, with the assurance that it was scutellaria galericulata, with a bulbous root; and had, by being used in mistake for the S. lateriflora, last spring, produced dangerous effects. It is however worthy of remark, that neither the one nor other of these plants, M. ringens nor S. galericulata, has a bulbous root, nor is either capable of producing any dangerous effect. The latter has been already stated to be a good bitter, and the other is absolutely inert.

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* A remarkable instance of this, is evidenced in the powers attributed to the scull-cap in a note to the case of Mary Tice, published in the Evening Post, 26th of August, 1820. It is as follows: "The great powers of this plant were mentioned to me" (the Editor of the Evening Post) "by two persons who each took too large a dose of it as a preventive, in case they had been endangered by her (Mary Tice's) saliva, the one a physician, and the other an apothecary; who both were much alarmed by the violence of its effects"!!! No comment on this is necessary.

The testimony, when such as to be relied on, evidently carries misapprehension with it, and describes imaginary hydrophobia or some hysterical affection.* I have not room in this paper to enter into an exposition of the numerous evidences of imaginary hydrophobia with which medical records abound; neither is it at all necessary. Suffice it to remark, that every intelligent and well informed practitioner has reason to believe that such have existed in very timid and nervous people, highly excited with the apprehension of disaster after being bitten by suspicious dogs. In *such cases*, it is my full belief, the scull-cap has been frequently administered, and *such* doubtless it has *cured*, aided, as the doctor, the patient, and the physic have all been, by the full confidence reposed in the curative powers of scull-cap, at least by the patient, if not by his medical attendant. A case of this nature occurred in my own practice, as will appear in the conclusion of these remarks. I shall now proceed to detail two cases, highly interesting in the present inquiry into the power of scull-cap. They so strikingly exemplify some of the preceding positions, that I hope they will supply what may by some still be deemed wanting in this paper, to complete the exposition of the futility of prescribing scull-cap with any other view than to profit by the fears of a patient relative to his apprehended disorder. This may, perhaps be justifiably done, by cajoling him into the belief, that, should he be so unfortunate as to become mad, in consequence of the bite of a dog (not rabid), he still has the consoling assurance that a *sovereign specific* exists the celebrated *scull-cap*. So far I am willing to profit by the *powers* of this plant; and for this purpose I recommend it to my brother practitioners, from the success I have had with it in the following instance:

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* For a masterly exposition of the misapprehensions and incredible circumstances appertaining to the case of Mary Tice, so much spoken of, and vauntingly declared conclusive of the curative power of scull-cap in hydrophobia, I beg leave to refer to a very long and interesting letter by Dr. William Tully, published in the Middlesex Gazette, of Middletown, Connecticut, on the 30th of November, 1820. The paper containing it, was put into my hands a few days ago by the Editor of this Journal; and, finding myself anticipated in my strictures on the abovementioned case, as well as in some remarks upon the case of John Cann, of New-York, by Dr. Tully's very sensible and excellent observations, I have suppressed my own remarks from this communication, as superfluous: and beg permission to request a perusal of that physician's letter, of all those who may read my remarks in this Journal, or feel interested in the subject.

Case I.

On the 20th day of July, 1819, at half past ten o'clock P. M. I was called upon by the wife of Philip Tunisson, (then living in Locust near Eleventh Street, but now in Market near Eleventh,) to visit her husband, who had been bitten in the arm half an hour before, by a neighbouring dog, supposed to be mad. Before visiting him, I requested her to conduct me to the house where the dog belonged, that I might ascertain his real situation. This she did; I saw the dog, which was evidently in perfect health. The person who owned him, an intelligent man, assured me there was nothing ailing him, and showed his opinion to be sincere, by suffering him to play with his children. Satisfied on this point, I visited Mr. T. and found him with a slight bite on the fore arm near the bend of the elbow. He was greatly agitated from fear of the consequences of this bite; and, though a courageous man, betrayed a degree of weak apprehension, which convinced me his family or neighbours had alarmed him. Though a robust man in health, his spirits were liable to sink in disease, as I had several times remarked in indispositions for which I attended him. I dressed the arm lightly, prescribed a saline draught, and quieted his apprehensions by stating the condition of the dog. The next day I visited him early; found the wound looking as contused wounds do, when inflicted on a person of full, vigorous and inflammatory habit; he had passed a tranquil night, and was without dread of any unpleasant consequences. At mid-day, however, he sent for me. I found him with renewed anxiety, his perturbation evidently owing to the officious cautions of his family and unfeeling remarks of his neighbours. Some one had informed him of the whole train of hydrophobic symptoms; the appearance the wound puts on when that disease follows the bite, the pains in the arm pits, shivering, &c. &c. and he endeavoured to persuade me that he had them all. In vain did I reason with him, or ridicule his fears; he became angry with me for so doing, and fearing the excitement of his system in consequence of his fears, might irritate or retard the healing of the wound, I soothed him with every art in my power, and then left him tranquil with a promise to see him at nine o'clock. He did not, however, await my visit, but becoming again alarmed, sent his wife for me at night-fall. I found, in her, the chief cause of his apprehensions; she was so much alarmed, that no persuasions could alter her belief that he was becoming mad; and she blamed me for not seeing things in the light she did. I accompanied her to see her husband,—found the wound doing well as before, and no symptom of disease about the man, except an obstinate conviction of his mind, that he was getting mad. I did not now leave him as before, with lessened apprehension, though much time was expended, and many arts tried to sooth him. I prescribed an opiate to compose him, dressed his arm, and promised to call at eight next day. He, however, came himself to me, in great perturbation, before that hour; described symptoms and feelings which I was convinced he had not, because I could neither see them, nor discover any evidences that they had existed. I sent him home with renewed assurances of his safety, and the good appearance of his wound and general health; enjoined rest, and the absolute necessity of dismissing his ungrounded fears. I did not see him again until four o'clock in the afternoon, when he was somewhat more quiet, finding the *hydrophobia* did not advance as rapidly as he had been taught to expect. At eight his wife again came to me and described his situation as quite critical; enumerated the symptoms before spoken of, as having supervened in an aggravated manner; declared she was afraid to stay in the house, lest he should bark and bite her; that he had himself the same fears, for the dog was certainly mad, &c. &c. I had again visited the dog; he was well.

Wearied with contending against such absurd apprehensions, it suddenly occurred to me, to change my conduct. I did so—required of her to repeat the symptoms, and then told her that upon re-consideration I thought they indicated something very like hydrophobia—but still, if he would follow my advice rigidly, I was in possession of an infallible specific for that dreadful disease. I desired her to wait in the entry where this conversation was held, and went into the parlour, where my friend Dr. Samuel Stewart was sitting. I related the circumstances briefly to him—told him if he would bear witness to my motives (which I assigned to him) for prescribing the scull-cap, I would give it to Mr. Tunisson. This he promised to do, and to remember the circumstance. I called in Mrs. T.—told her in the presence of Dr. S. that I was fearful, on reflection, from what she had said, that hydrophobia would supervene—but gave her the *scull-cap* (the recent plant of which I had collected a quantity with a view to make a drawing,) and put into her hands a long account in the Evening Post of the 16th of July, which I had on my table—desired her to read it to Mr. T. give him a strong decoction of the herb as directed in that paper, and I doubted not, by the time I should see him next day, the symptoms would have left him. She seemed greatly pleased, and much relieved of her anxiety for her husband, my manner having been so grave that she had not the slightest suspicion of any finesse or insincerity. Dr. Stewart and myself amused ourselves on the subject when she left us; and I predicted to him a *perfect cure* by means of the *scull-cap* and *newspaper*. The result verified my predictions. Mr. Tunisson came himself to me next morning, while Dr. Gibson was in my office, declaring the *medicine* had acted like a *charm*, that he was *perfectly well*—never better in his life—pains in the wound and arm pits gone, &c. I directed attention to daily dressing his wound (now nearly healed), and a recurrence to *scull-cap*, should he have a *return of the symptoms*, which had alarmed him, but which I assured him seldom if ever returned, after taking a single draught of scull-cap. I visited him every day or two—and never found him suffering from a moment's anxiety. He had no idea, and to this day has none, that I practised on his fears, and thus cured him. With more credulity than I possessed, some persons would have believed this man's case one of true hydrophobia, or the commencement of that disease; and with more reliance on the public newspaper accounts on the subject, and less information than experiment had given me, on the subject of the medicinal powers of scull-cap in relation to it—would have attributed the cure to the administration of that herb; or would, at least, have declared the approaching symptoms of hydrophobia had been unequivocally arrested by its prophylactic powers. I do believe such cases as this have been mistaken for hydrophobia, when, though the dog has not been rabid, the patient could not be persuaded of the fact, and, by suffering his apprehensions, aided by the officious interference of friends, to master his judgment, has worked himself into a degree of mental agitation, which, under other circumstances would not have occurred. Yet it is evident, that the fears of the patient are not to be disregarded, since they can produce a hysteric disease like the preceding. But, if they amount to such a height that we cannot by reason, convince them—the aid of prejudice may be resorted to, and the fancies of the patient may profitably be enlisted, to sooth his mind and remove his apprehensions.

Case II.

At ten o'clock P. M. on the 28th of November, *1818*, I was called to visit John Wolman, Quince Street, near Locust. The messenger had left the call on my slate half an hour before, so that I had no intimation of any thing very particular in the case of this patient. He was a short athletic man of about forty years of age, of temperate habits and courageous disposition, and had a wife and several children. On entering the room my eye met that of this man, who was lying on the outside of the bed, and discovered to me a very peculiar and embarrassing physiognomy. It was full of suspicion and lowering expression. He shuddered distrustfully at my approach to the bedside, for a moment, and then recovering himself, replied to my inquiries into his feelings, with quick and impatient clearness. He had a chill; complained of pain in the back of the neck, loins, head, and stomach particularly. Said his throat was *dry*. His tongue was foul, pulse full, very hard, and remarkable for vacillancy between ordinary quickness and febrile frequency. Respiration natural. He was greatly depressed in spirit, so much so, that this circumstance, after learning from his wife his usual cheerfulness, together with the remarkably distrustful physiognomy already mentioned, caused me to be very anxious, and indeed, on reflection—*strangely* apprehensive, for his situation otherwise, presented nothing different from the ordinary mode of approach of fever. I endeavoured to dissipate this unaccountable dispondence. He hastily turned from me, saying, "You cannot relieve me; I am not for this world." I bled him, administered fifteen grains of calomel with ten grains of calcined magnesia, and left a solution of tartar emetic, to be taken after the cathartic had ceased to operate, or in case it should not move the bowels before five o'clock next morning, ordering him lemonade drink during the night. He made no objection to this beverage, but indeed *preferred* it to tamarind water or treacle, which were also proposed. He was entirely submissive to the remedies; showed no repugnance at the idea of swallowing fluids while I was there, though I remarked, when his wife, at his *own request*, presented a tea-cup of water, of which he quickly sipped a little, that deglutition hurried his respiration for a few seconds. Though obvious, this effect did not impress me with any suspicion as to the real nature of the man's disorder, but appeared to me to partake of the general flurry of his actions, which might be characteristic of his common behaviour.

At seven o'clock next day, (the 29th,) I was sent for to see him without delay, the messenger informing me that he had been very restless and troublesome all night; not only refusing to lie down, but frequently starting from the edge of the bedstead on which he occasionally sat, and traversing the room with perturbation. I found him in a state of frightful excitement and muscular mobility, with a wild, protruded, staring, but unsteady eye. The disturbance of his mind, evident on my first visit, was increased to the most distressing agitation. He started from the door, near which he was standing when I entered, being either worried at seeing me, or disliking the rush of cold air from the entry—stood for a few moments at the window, with his back towards me, looking, by sudden and apprehensive glances over his shoulder towards the place where I stood making inquiries of his wife and attendant; and when I finally addressed myself to him, to ask him how he felt, he replied by very quick and abrupt, though rational answers. I learned from his wife that his physic had operated freely, and he had been puked once by the tartar emetic, discharging a large quantity of bile and much ropy mucus, which he repeatedly pulled impatiently out of his mouth with his fingers, during the operation of the medicine. That an hour after I left him the preceding night, he suddenly jumped up from the bed on which he had been tossing about, and complained of intense burning in his throat, and appeared to be oppressed with phlegm. She offered him a drink of lemonade, which he instantly flung from him with disgust, and was seized with the most alarming spasms, which threw his head backward, and seemed near "choking him;" that he made no remark about the lemonade; and she, attributing his conduct to her having suddenly presented the drink, without being asked for it, again offered it, on his complaint of intolerable thirst. As before, he was seized with spasm ("fits") and "choking," and shouted violently that "she wanted to kill him." During the operation of his cathartic, forgetting, or not attributing to the right cause, his abhorrence of the lemonade, she told him he had better "drink something to help him," when he abruptly jumped from the close-chair, screamed to have the door opened; was seized with "choking" and spasm ("fits") and hastily flung it shut. That he seemed gasping for breath, and raised the window himself, but instantly let it fall and strode repeatedly across the room. That during the whole night he walked to and fro, suffering much from a difficulty of breathing, which was fitful, for at times he was calm in respiration: expressed great surprise at "what could ail him;" apologized for having spoken roughly to her; saying he verily believed he was "out of his head." In this state of mind he reached the day-light, when I was directly sent for. His behaviour on my entrance has already been mentioned.

It is needless to say that the moment I saw him this morning (the 29th) and heard but part of the information above detailed, I made up my mind as to the nature of this poor fellow's disorder. As it was not difficult either, to discover, that a few hours would in all probability end his sufferings;

and as his wife was in hourly expectation of labour, and seemed in great affliction, I was unwilling to make any inquiry to ascertain if he had received any injury or bite from a mad dog. I was confirmed in my determination, by the conviction of my own mind that the disease was hydrophobia, whether supervening symptomatically on the bite of a rabid animal, or whether occurring spontaneously as an anomalous symptom of fever, with great tendency to nervous irritability—and also because my practice would not, in this stage of the disease, have been in any way regulated or altered by any further information—and I wished to hear, without prejudice or misrepresentation, the symptoms, or any change in them, which might occur during the intervals of my absence. Not the slightest suspicion of the real nature of his complaint existed either in the minds of his wife, or relations attending, nor did the poor fellow himself appear to have any idea of the cause of his disease. Hence I scrupulously avoided any hint on the subject to him or his friends. I had not a moment's hesitation in repeating a very free bleeding, and administering a scruple of calomel. This was swallowed in a bolus after the greatest difficulty, and after several fruitless efforts to take it into the mouth, each of which produced a distressing sense of choking. It was finally accomplished by the firm resolution of the patient himself, who, more tranquil from the bleeding, stated his willingness to do any thing that "would not choke him," for relief. He declared he could not tell why he abhorred the lemonade or some tea spoken of, but not presented to him, for he was intolerably thirsty; but that an attempt to take it, seemed "like to kill him." I reasoned with him, and asked him to swallow a tea-spoonful only of water. He said he would do so, if his wife would hold him round the waist. The instant however that I presented this small quantity to him, the most horrible spasms ensued—his bleeding seemed no longer to compose him. He ran into the yard, opened the privy door, violently shut it again—returned to the apartment, opened the stove, closet, and front door, in the same wild, abrupt and vehement manner, closing each with a sudden jerk, as if he had seen behind each, a spectre. He now spat frequently and abruptly, about the room, on the table, the stove, the victuals which were on it, and on his own clothes, and those of his wife, brother and myself. But, so far from designing to injure any one, was evidently timorous to a morbid degree, looking imploringly, but suspiciously and wildly, at every one but his wife, and particularly at me. On questioning him in a more tranquil moment why he was fearful we would do him harm, he said he "could not tell" —that he believed he "was out of his head"—and that the reason why he strode from place to place was, that he felt as if he was "choking." He was as much affected in respiration and spasms, by a sudden rush of cool air, as by an attempt to swallow water. His enunciation, I was informed, was in health slow and distinct. He now answered questions with inconceivable rapidity and vehemence, and with an indistinctness, (clipping his words like an intoxicated man,) evidently owing to a want of power over the organs of speech, as well as to the quantity of viscid mucus which seemed to clog up his mouth, fauces, and throat.

In an hour I returned, and administered two drachms of the finely powdered leaves, stems, capsules, and seeds of scull-cap (S. lateriflora.) This was taken with extreme difficulty in a table spoonfull of coffee, and, as before, after several fruitless efforts, but not until the man had summoned all his resolution, and was held by his wife as before. It produced spasm and hurried respiration. In half an hour three drachms more were, with somewhat less difficulty, swallowed, and, in the course of an hour after, two boluses, each containing eight grains of the pulverized leaves, mixed with conserve of roses, were given. The extreme agony induced by swallowing them, made the patient rebellious at any further attempts to exhibit liquids or solids of any kind. The morbid irritability before mentioned, was again conspicuous, accompanied by an excessive muscular mobility, which suddenly and vehemently exhibited the impressions made by those causes which most affected this irritability. These were cold air, the presentation of lemonade, water, coffee, or treacle to him to be drunk when he complained of thirst; suddenly approaching him or abruptly interrogating him as to the seat of his sufferings or his sensations. Yet it was remarkable that this man was never entirely without reason or reflection; and in the absence of the hurried spasmodic respiration, which always came on by paroxysms, and was evidently produced by irritation in the fauces, throat, and œsophagus; he often complained of acute pain in his stomach, but most constantly of *intolerable thirst;* this never left him, except for an hour at a time. In the afternoon I profited by it, and urged his taking lemonade, assuring him in the most solemn manner, I would not present it, until he himself should say he was ready to swallow it. I poured two ounces of a saturated brandy tincture of scull-cap into a large tea-cup full of lemonade, and awaited his request for drink. Repeatedly did he, with the most distressing efforts at resolution, tell me he was ready; fix one hand firmly on the back of a chair, and suddenly ask for the cup. As his countenance seldom, in these attempts, showed his mind fixed, I very slowly brought it towards him, and when near his hand, he turned himself away by a violent jerk, running towards the door or window, quickly opening and as quickly clapping them shut again. I however persevered in my solicitations; and finally he entwined one arm around the waist of his wife, who did the same with him, fixed the other energetically to the back of a chair, and asked her to administer it. This she did, and after the first spasm was overcome, he gulped the whole quantity of fluid without stopping, but instantly was seized with tetanic retraction of the muscles of the back and nape of the neck, and, vociferating indistinctly, threw himself violently backward on the bed. The spasm went off; he seemed more tranquil; jumped up and slowly traversed the room; described the effect of the liquid on his system, by saying "it stops up the way, and drives the breath out of my body. God knows how much I want to drink, for I am burnt up in my insides; but it chokes me. I cannot, will not, take any more." I now left the house, and did not again see him till dusk, when I found the calomel had purged him copiously. He had walked to and fro the whole afternoon, shouting violently, and asking vehemently "who's there" at every noise which was made like a person knocking at a door, whencesoever such noise proceeded. Even the rattling of carts, I was informed, caused this question. He dreaded my approach; declared he would not see me, and when I finally arrived, and knocked at the front door, he bellowed frightfully, as I myself heard, and ran up into the third story. Unwilling to aggravate his fears, I waited some time below, begging his wife to persuade him to see me, and to assure him of my kind intentions. She returned, saying he threatened to jump out of the window and destroy himself, if I or any other person was admitted to the room. Entreaties having no effect, I went unexpectedly up stairs into the room, and was shocked at the condition of this poor fellow, which was something between maniacal and rational, or perhaps both, by fits. He flirted from me to a corner of the room, and finding I did not follow him, became tranquil. In a few seconds he permitted me to feel his pulse, which was tense and very frequent. His face was purplish-red; his eyes protruded and blood-shot; his tongue dry, and not the least portion of mucus or saliva discoverable in his mouth.

I proposed bleeding, and offered to do it. He shouted, ran from me, and was so much perturbed that I left him, informing his wife I would get another physician to see him with me, requesting in the interim to send for her brothers, to assist in holding him if necessary. I called on my brother Dr. John Rhea Barton, but finding him out, I requested my friend the late Dr. Miller,* of the Pennsylvania Hospital, to accompany me to see the case, and assist in bleeding, which he directly did. Our entrance terrified the patient, though in the third story; and it was twenty minutes before he could be persuaded to admit us. He was more quiet, and had again been purged by the calomel a few minutes before. He would not permit either of us to take blood, and we left him, desiring a bleeder to be sent for, and a sufficient number of persons to overcome his resistance. I directed his brother to send for a bleeder also at day-light, and again have him bled, in case he was not more tranquil during the night. I visited him at eight o'clock next morning, November 30th, and found every symptom aggravated to a painful extent. He had traversed the room the whole night—half delirious, but not attempting to hurt any one. On the contrary, he feared harm of all who approached him, except his wife and children. The secretion of tough, viscid frothy mucus was now increased to such an extent, that he could only get it from his mouth by flirting it out with his fingers, spitting suddenly about him on every thing in his way. He had not been bled as ordered, at day-light; I therefore directed the bleeder to be sent for—and prevailed on him to swallow two pills of seneka.* On my return in half an hour, the bleeder was taking blood from him, and the patient was greatly terrified, more by the number of persons who had come in to hold him, for the operation, than from any other cause. I am quite confident that the dribbling of the blood into the basin, did not excite any

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* The following is Dr. Miller's statement of his appearance at that time:

Pennsylvania Hospital, Nov. 30th, 1818.

DEAR DOCTOR,

The following are the most obvious symptoms of the patient you were so polite as to permit me to see, yesterday evening.

Respectfully,

W. P. MILLER, M. D.

DR. WM. P. C. BARTON.

"On entering his room, about nine o'clock P. M. I found him reclining on his bed, his face flushed, his eyes suffused, respiration natural, pulse quick and tense, mouth dry, and his tongue covered with a white fur.

When he was requested to take some drink, he expressed the utmost horror, and refused to take any; his wife however presented him with a cup of lemonade, and after much persuasion, he seized it, passed it to his mouth, and swallowed a few table spoonfulls with the utmost difficulty. He appeared to be rational, complained of having been delirious; his conversation was occasionally interrupted by spasm about the throat, which he attributed to wind rising from the stomach, almost strangling him. Just before my leaving him, his wife gave him a cup of water; he swallowed some, and ran across the room with the utmost distress in his countenance, as if to avoid strangulation."

* See my Vegetable Materia Medica, vol. ii. page 123, article Polygala Senega.

horror, or disgust, or spasm—but he was evidently timorously uneasy while it flowed, looking frequently and suspiciously at the bleeder, his arm, the blood, and all around him. He was now relieved, and somewhat prostrated, but pulse full and hard. He refused *with horror*, and frightful physiognomy, all kinds of drink, though complaining still of thirst. Took powders and pills for three hours, of seneka, which I prescribed with a view to promote a free expectoration. This they did, though not administered to any great extent. On my last visit, at half past eleven o'clock on the 30th, I found him in the room on the first floor, surrounded by many persons, who had at different times been called in to hold him. He ran into the yard, opened the privy door, returned suddenly, strode up and down the room, evidently terrified—spoke thick and incoherently, was seized with violent spasms about the throat, fell on the floor and puked up in large quantity a matter resembling black vomit of yellow fever, and other malignant diseases—was exhausted by the effort, and respiration becoming more difficult, a severe spasm put an end to his sufferings. He had lived in this distressing torment nearly two entire days, during the whole of which time he had not slept for one minute, nor been quiescent for more than ten minutes at a time. That gastric disease produced this dark discharge, I cannot doubt; for the patient complained at times severely of pain in his stomach and gullet.

I wished to make an examination of the body—and accompanied by my brother Dr. John Rhea Barton, I called in the afternoon for the purpose of doing so. We were unable, however, by our joint persuasions, to induce the relations, now all collected and terrified, to consent. From the latter I made inquiries relative to any accident he may have received, and learned this history from a coachman, now living with Mr. Lyle at the Woodlands: Mr. Wilson Hunt, merchant of this city, had a terrier dog, which behaved *suspiciously*, in consequence of which, Mr. Hunt ordered his groom to destroy him. The latter not liking the office, and not thinking the dog mad, as his master did, employed Wolman to do it. They, with an assistant, proceeded to the commons, and with an ax killed him; not, however, before Wolman, in the struggle, was bitten in the finger. At this time he said, "if this dog is mad I shall be mad too, for he has bitten me." The wound was washed with salt and water, and no more thought about it. This was three weeks before I was called to see him in hydrophobia. It may be here necessary to state, that neither Wolman, nor the assistant, any more than the groom, believed the dog mad; though the greater discrimination of the master, Mr. Hunt, saw enough aberration in the animal from his customary behaviour, to *insist* on his being killed. This account was given to my brother and myself, at the door of the house, by the abovementioned coachman. We therefore immediately re-entered and examined the corpse. A squamous kind of cicatrix was found on the finger designated, which was manifestly that of a recent wound. On appealing to his wife for further information, she confirmed what had been told, with surprise that the nature of the disease had not occurred to her, for she declared that for more than two weeks he had "*constantly picked at, and rubbed that finger*" between those of his other hand. Mr. Hunt's groom, an honest mulatto, and then my patient, confirmed, in half an hour afterwards the preceding statement.

In this case, scull-cap (S. lateriflora) was given in substance to the extent of one ounce and a half, and in tincture to the extent of two ounces, twelve hours after the disease had supervened, and without the slightest alleviation of the peculiar symptoms of the disorder. It was, in fact, subjecting the patient to an unnecessary aggravation of his pain, by forcing him to take it; but I wished not to lose the chance of a trial, and even thought I should be unjustifiable had I not done so. I ought not to conceal, however, that I had no confidence in the virtue of the plant at that time, and when urged to give it by Dr. Eberle, as an experiment, I explicitly stated to him, my want of faith in this article. His solicitations, and my own reflections determined me to alter my mind.

I cannot end this communication without an apology to the members of the profession at large, for having said so much on a subject, on which, measured by its merits, so little ought to have been said; nor can I refrain from deprecating their censure, for having been at so much pains to convince them of the intrinsic immerit of an article, which has surely never had the confidence of one among an hundred intelligent and reflecting practitioners. Though I have in the commencement of this paper, declared fully my motives for appearing in public on the subject, I again aver, that nothing would have induced this step, but the idea that the number of believers in this preposterous story, was gradually becoming larger, owing to the pertinacity of those who keep it before the public in a perpetual glare of of varnish. Their statements imperiously required a refutal from some one of the profession; and feeling much interest in enlarging and enriching, and very solicitous to expunge useless articles from, our indigenous materia medica, in the investigation of which I have been avowedly engaged, I felt it a duty* to obey this requisition.

One more observation and I shall close this paper, already extended beyond the limits I had calculated. Anxious that this subject should be fairly and fully known, I have, simultaneously with the publication of these remarks, published a full botanical history of the scull-cap, accompanied by a coloured engraving, in the February number of my Flora of North America. To this I refer, for such information as will enable any one to identify and collect the plant for experiment, repeating my assurance of willingness to aid in this whenever called on so to do.

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* I have performed this duty of calling the serious attention of physicians to the real character of this boasted specific the more willingly, because I have been instrumental, in some degree, in bringing our native materia medica prominently before the public. In doing so I feel conscious of having desired to restrict myself to the history of those plants which enjoyed, or deserved to enjoy, the confidence of the profession, being always solicitous to expose, as I have done in the preceding remarks, the worthlessness of inert articles. This circumspection it is the more incumbent on any one in the investigation of an indigenous medicine to practise, because there are not wanting in our medical schools professors who are opposed to such inquiries, believing that they can only lead to a superfluous multiplication of the articles of the Materia Medica, already loaded and redundant, than which, in my opinion, no reasoning or views can be more fallacious or unfounded, as I hope hereafter to show.

CASES.

Art. IX. *A Case of Inguinal Aneurism, communicated by* Nathan Smith, M. D. Professor of the Theory and Practice of Physick and Surgery in Yale College.

ACHILLES H. ELLIOT of Killingworth, Connecticut, some time in the month of June last, felt a pain in the left groin, and soon perceived a small pulsating tumor: it was situated just below Poupart's ligament, and increased pretty rapidly. On the 25th of July, with the assistance of Dr. J. Knight, professor of anatomy in this institution, I undertook the operation of *tying the external iliac artery.* The aneurismal tumor was situated in the left groin. It pulsated strongly, and from its size, it might contain eight ounces of blood. The whole limb was considerably œdematose. We followed Dr. Dorsey's mode of operating in this case, which is not materially different from that of Mr. Abernethy, who was the first that attempted this operation, and to whom we are so largely indebted for other important improvements in surgery.

The patient being placed in a proper situation, I commenced an incision, about an inch above Poupart's ligament, and directly over the external iliac artery, extending it obliquely upwards, nearly in the direction of the fibres of the external oblique muscle, about three and a half inches. The skin and adipose membrane being cut through, the fibres of the external oblique were divided longitudinally, and the internal oblique and transverse cut across, which brought the peritonæum into view. I then insinuated my finger between this membrane and the muscles down to the external iliac artery, which I readily found, and endeavoured to detach from the surrounding parts. By means of Dr. Physick's curved forceps, I then passed the aneurismal needle under the artery; but owing to an imperfection in the needle, which was made on the spur of the occasion, it slipped out of my fingers, after I had hold of the point of it. I was obliged to withdraw it and fix it again in the forceps, and when I brought the point out a second time, Dr. Knight took hold of it with a firm pair of forceps and drew it through, which carried the ligature under the artery. After examining, to be certain that the ligature included nothing but the artery, I drew it very tight. At this moment the patient complained of severe pain, but it lasted only a few minutes. The pulsation in the aneurismal tumor ceased immediately on tightening the ligature.

The wound was then dressed with adhesive straps, with a compress and flannel bandage over the whole, adjusted in such a manner as to afford moderate pressure on the wound and aneurismal tumor. Before we had finished the dressing, the limb of the affected side was sensibly colder than the other. The operation was performed about ten A. M. At two P. M. the left limb was warmer than the other; in the evening I could perceive no difference in the temperature of the two limbs. The affected limb as well as the whole body was in a state of perspiration, the weather being very warm at that time. The next day a pulsation was felt in the posterior tibial artery, where it passes under the maleolus internus, and the aneurismal tumor was considerably diminished. It continued to decrease, until it wholly disappeared, which happened in about four weeks. The ligature came away on the twentieth day. There was not the slightest hæmorrhage, and no unfavourable symptoms occurred during the cure. The limb was somewhat debilitated at first, but it has now entirely recovered its power. The ligature made use of in this case was linen shoe-thread, made small and hard, and I endeavoured to draw it so as to kill the artery in that point on which it acted. I tied with a single ligature, and did not divide the artery.

There was not the least embarrassment in the operation, except what arose from the imperfection of the needle. To remedy this in future operations of the kind, the needle should have a broad point, and so curved, as to describe nearly half a circle in its shape.

Yale College, Dec. 16, 1820.

Art. X. *A Case of Luxation downwards of the Os Humeri, communicated by* Charles Caldwell, M. D. &c. &c. *to the Editor.*

THIS interesting case was very dexterously and successfully managed by Simeon A. Dudley, M. D., a young physician of great discernment, enterprise, and promise, in Winchester, in the state of Kentucky.

The resistance experienced by surgeons in their efforts to reduce luxated bones, before morbid adhesions have been formed, arises almost exclusively from the excessive and obstinate contraction of the surrounding irritated muscles. To attempt to overcome this resistance by mechanical force, is always productive of pain, and oftentimes of danger. A leading indication then is to procure a relaxation of the muscles in action. It is scarcely necessary to add, that the more complete this relaxation becomes, the greater will be the facility in the process of reduction.

It would be superfluous in me to enumerate the various expedients usually employed for the attainment of the indication in view. They are familiar to every one even moderately versed in the practice of surgery.

It will be permitted me, however, to observe, that whatever makes on the stomach a deep and highly deleterious impression, diminishes or destroys alike the power of contraction in the muscles, and of coagulation in the blood. In cases of sudden death from a violent blow on the scrobiculus cordis, from the intemperate use of cold water, from an excessive draught of ardent spirits, or from the swallowing or receiving by injection of a large amount of certain vegetable poisons, such as laurel water, or a strong decoction of tobacco, the muscles can neither be made to contract, nor the blood to coagulate. These facts being known, the intrepid and judicious practitioner, accustomed to the apportioning and management of powerful remedies, and skilled in the adaptation of means to ends, will readily avail himself of them in cases of emergency. The truth of this, the case in question, of which the following is a brief narrative, illustrates and confirms.

On the first day of October, 1818, A. J., a labouring man, about thirty years of age, and of intemperate habits, experienced a luxation downwards of the os humeri. By several physicians in the neighbouring country, both singly and associated, efforts at reduction were immediately, repeatedly, and perseveringly made, but without success.

About eight weeks after the accident, Dr. Dudley's aid was solicited. High spirited and fearless, the patient declared himself willing to submit to any suffering, and even to hazard his life for the restoration of his limb. His physician, no less intrepid, was equally ready to encounter a weighty and generous risk of his own reputation to effect a cure, a duty which, in desperate cases, every practitioner owes alike to his patient and his profession.

All the most powerful ordinary means, such as deep and long continued nausea, blood-letting ad deliquium animi, the warm bath, forcible extension, &c. were first repeatedly and faithfully essayed.

These proving fruitless, a resolution was formed to have recourse to the effects of *deadly intoxication.* Comporting with habits in which the patient had too long indulged, this was not, in prospect, a disagreeable remedy.

Early in the morning the copious potation of rum, brandy, whiskey, and gin was begun, and by ten o'clock, A. M., such is the force of habit, that three pints of these liquors had been swallowed, without producing the desired effect, or even any very obvious approach towards intoxication. At this period another half-pint draught was administered, consisting of equal parts of rum and whiskey, as hot as it could be endured. For a few minutes this also was without effect. But at length the patient's whole energy suddenly deserted him, and he fell as if pierced by a bullet, or striken by a flash of lightning. His stupor and insensibility were deep and alarming, far beyond what the spectators had ever witnessed as the effect of liquor.

Not a moment was to be lost. Now, if ever, was the time for the reduction of the dislocated bone. After several efforts, in which the strength of six powerful men were employed, this was at length effected.

But new dangers now presented themselves, and new difficulties were, therefore, to be surmounted. So entire was the prostration of all the vital energies of the patient, that he was evidently hovering on the brink of dissolution. His sensibility was extinguished, his respiration scarcely perceptible, the pulse in his extremities not to be felt, and his skin was cold, clammy, and cadaverous.

Deglutition being now impracticable, and injections not deemed advisable, external applications were alone employed. So wholly inadequate were these for a time, that even *boiling water* applied to the feet and ancles, and aided by strong and continued friction, produced no effect. It did not even redden the skin.

Under every external impression that could be made, this state of things continued for ten hours, when symptoms of returning health began to appear. In twenty hours more the patient had recovered sufficiently to ride home on horseback, a distance of ten miles, and in three weeks was well enough to resume the labours of a country life.

For the individual concerned, as well as his family, the most fortunate issue of the process is, that his habits of intemperance are completely eradicated. His love of ardent spirits is converted into an inconquerable dislike of them.*

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* It is due to Dr. Physick, to state that the practice of inducing intoxication with a view of relaxing the muscles in obstinate luxations, originated with him, and, we have no doubt our ingenious correspondent would have had pleasure in conceding to him this praise had he been apprised of the fact. Vide Dorsey's Elements of Surgery, vol. i. p. 204. EDITOR.

REVIEWS.

ART. XI. *Flora of North America, illustrated by Coloured Figures, drawn from Nature.* By W. P. C. BARTON, M. D. U. S. N. Professor of Botany in the University of Pennsylvania. Philadelphia, M. Carey & Son.

TO the cultivators of American Botany, Dr. W. P. C. Barton is well known, as the author of several valuable works, all of which give eminent proofs of his industry and ardour, and of a mind and taste peculiarly suited to this fascinating study. His "Vegetable Materia Medica" more especially has acquired considerable reputation and is undoubtedly one of the most elegant works, which have issued from the American press. The present is an enterprise of a much more extensive nature, and though less interesting to the merely medical man, promises to be much more so to the lovers of Botany both in this country and in Europe. The importance of accurate delineations in the determination of species, is readily appreciated by those, who are aware of the uncertainty which attends the comparison of our plants with their European congeners, when aided only by the short technical descriptions of Systematic Botanists. The concise language of Botany, though strikingly exact, and perhaps sufficiently expressive of generic characters, is less applicable to specific differences and quite inadequate to convey an accurate idea of what may be termed the habit of the plant.—The uncertainty, arising from this circumstance, has been increased by the change produced in the aspect of many of our vegetable productions, when transplanted from their native soil and climate into European gardens. Hence many of our species have been considered doubtful, new ones have been formed, which have afterwards been found to be identical with long known European species, and others have been confounded, which were specifically distinct.

To remove these difficulties, and to " elucidate the whole of the Botany of North America by a coloured figure of every plant, on the plan of the Flora Danica, is the design of the present work." The author announces in the advertisement his intention of publishing a number every month, so that a volume containing thirty-six plates, will be annually completed. Five numbers have already appeared, containing descriptions and coloured figures of the following plants; Lysimachia racemosa—Scutellaria Hyssopifolia—Erythrina herbacea—Rhexia Virginica—Rudbeckia fulgida—Œnothera grandiflora—Pinckneya pubens—Linum Lewisii—Hibiscus speciosus—Echites difformis—Chimaphila maculata—Coreopsis rosea—Schisandra coccinca—Ludwigia macrocarpa—Orchis tridentata.

In place of the Scutellaria Hyssopifolia, it would have been more judicious to have given a plate of the S. lateriflora, concerning which popular curiosity has been of late so much excited. An accurate figure of this plant is still a desideratum, and the public confidence in it as a supposed remedy for Hydrophobia will never be dissipated, so long as its failures can be attributed to mistakes in the species. Of its efficacy however in the cure of this dreadful malady, most physicians we suspect are as sceptical as Dr. Barton himself.

In the third number we are presented with an excellent figure of the Linum Lewisii of Pursh, which most of our Botanists have thought improperly separated from the L. percune.—Dr. B. has satisfied himself by comparison of the American plant with a dried specimen of L. percune, that they are specifically distinct.—This decision from the examination of a single dried specimen is perhaps too hasty; most botanists, we are inclined to believe will concur with Mr. Nuttall in making it only a variety of the European plant. Mr. N. cultivated the two together, and is convinced by comparing the living plants, that there is not sufficient ground for separating them into two species. Dr. B's figure is strikingly characteristic of the habit and appearance of the American plant.

The detailed descriptions, which follow the generic and specific characters of each plant, are drawn up with great care and fidelity. The only objection indeed, which could be urged against them, is the minuteness, with which the author has noticed circumstances, which vary in each individual, and cannot consequently be characteristic of the species. The specimen before him seems rather to have been the object of his description, than the species to which it belongs.—In defining the colours of the flowers, leaves, &c. the author has adopted the nomenclature of Werner and Symes, which contributes very much to the precision of this part of the description.

The plates, which constitute the most attractive feature of the present work, are executed in a style which merits the highest commendation. They are characterised by the same accuracy of delineation, and beauty and correctness of colouring, which were remarkable in most of the plates of the Materia Medica. There is indeed a manifest improvement in the drawings, and it is but justice to say that they are superior to any thing of the kind, ever published in this country.

We have no doubt Dr. B. will receive from the American public such encouragement as will enable him to continue this splendid and arduous undertaking, which, when complete, will form a national work, highly honourable to the enterprise and talents of the author.

ART. XII. *Treatise on the History, Nature and Treatment of Chincough, &c.* By ROBERT WATT, M. D. Member of the Faculty of Physicians and Surgeons of Glasgow, Member of the London Medical and Chirurgical Society, &c. and Lecturer on the Theory and Practice of Medicine in Glasgow.

> —— "quæque ipse miserrima vidi,
> Et quorum pars magna fui." *Virgil.*

ENGLISH Medical books come to us at a price so enormously high, as to amount nearly to a prohibition. Excepting some few of the wealthier members of the profession, no one among us can afford, or even thinks, of making such purchases to any extent.

It is really to be regretted, that so serious an impediment exists to the diffusion of the publications of that country, as many of them of late are excellent. Discarding the absurd notions of *debility* and *putrescency*, so long dominant in their schools of medicine, and which were inconceivably pernicious in all their tendencies, they seem, in recovering from the delusion, to have embraced far better views of pathology, and as the natural result, a more vigorous and efficient system of managing diseases.

Nevertheless, to the well educated and intelligent American physician, we are not aware, that many of their works on merely practical subjects, afford much absolute novelty. It will, indeed, be among our earliest undertakings, to show, that in the great revolution which has taken place, in this respect, we led the way, and have a clear and indisputable title, to no small portion of the improvements, of which they boast.

Yet, it must be confessed, that, in regular learning, in diligent and recondite research, in the power of writing, as well as in the whole art and mystery of book making, they are incomparably our superiors. It is on this account, that their writings, exhibiting such extensive and various information, would be sought after with eagerness by us, were not the cost so incommensurate with the means of the profession at large.

Considerations of this description, have led us to believe, that we might probably be of service to our readers, whose remoteness of position excludes them from public libraries, and other repositories of such books, by occasionally presenting a sort of synopsis of the best practical productions, accompanied by the additional information which we may be able to supply, with an accommodation of the whole to the peculiar nature and condition of disease in the climate of the United States.

In our opinion, the common analytical mode of reviewing, here and there, extracting a passage as a specimen of a work, is a very idle process, utterly unproductive of usefulness. It does indeed always remind us, of the story of the silly man of antiquity, who having a house to sell, went about with a brick in his hands, to show as a sample of the building. There is no essential difference in the two cases, as to the merit of the device.

Of the work before us, we may state generally, that it is the most erudite and comprehensive treatise which we have met with on hooping cough. To the investigation of the disease, it appears, the author was instigated particularly by having had the misfortune to witness a distressing degree of mortality from it in his own family. Distrusting, in consequence, the correctness of the prevalent views in relation to it, he resolved to avail himself of the advantages he possessed to explore anew, the subject, and by a series of *post mortem* examinations, has undoubtedly elucidated more distinctly its pathology, and perhaps prepared us to arrange and digest a more definite and successful plan of practice.

Besides pertussis, the common nosological title, various other appellations have been applied to the disease, as tussis convulsiva, tussis clangosa, tussis perennis, tussis amphimerina, tussis suffocativa, tussis puerilis tussiculosa, and in vulgar language, kinkcough, chincough, and hooping cough. The first of these familiar names, *kink*, is a Scotch word, signifying paroxysm or fit, the second is a corruption of *chain*, from a notion that the disease had its seat in the spine, the several parts of which were supposed to be linked together, and the third is obviously from one of the more prominent symptoms of the complaint.

The disease is of modern date. No notice of it is contained in the Grecian, Roman, or Arabian writers. Towards the middle of the seventeenth century, it was first regularly described, by Willis, and soon afterwards attracted the attention of his cotemporary Sydenham, by whom it is incidentally mentioned, in his account of the measles, of sixteen hundred and seventy.

The hooping cough generally begins like a cold, with more or less fever, and catarrhal affections, which continue in some instances throughout every stage of the complaint, while in others they cease in a few days, and most always when the hoop supervenes.* The onset of the disease, is for the most part abrupt, and is sometimes early attended by the sonorous spasmodic inspiration, which confers on it the most popular name. But at other times, a considerable period elapses before it takes place, and in some cases does not at all happen. The average period of its occurrence, is about three weeks.

After the disease becomes confirmed, the paroxysm consists of a number of short expirations closely following each other, so as to produce a sense of suffocation, to overcome which, a violent effort of coughing is made which usually ends in vomiting, or a discharge of phlegm from the lungs. The paroxysm being over, there is, with the exception of some temporary exhaustion, complete relief in the interval, so much so, that the individual seems not at all affected.

Expectoration, in the commencement, is very deficient, the cough hard and dry, and at this time, the paroxysms recur frequently, and are long continued. Congestions of the lungs now take place, which produce, by the interruption of the circulation in these organs, a correspondent state of the head, and as a consequence, a turgescency and suffusion of face,* amounting sometimes even to lividness, which is relieved by gushes of blood from the mouth, nose, eyes, or ears. In this manner, the disease runs an indefinite course, from one month to three, or even twelve months, though the average is, perhaps, the second period. This, however, depends much on the season of the year, it being always most difficult of management, and hence is of a longer duration in winter. The popular notion is, and which is not far from the fact, that it is three weeks in reaching its height, continues three weeks with little abatement, then declines, and goes off in three weeks more.

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* "I have had instances of a disease," says Cullen, "which though evidently arising from the chincough contagion, never put on any other form than that of common catarrh."

This disease is generally bad in infants. Though milder in them, they cannot expectorate, and hence are debarred this source of relief. Where it attacks with much fever and catarrh, it is also unfavourable, and with peripneumonia notha still more so. Consumptive subjects rarely or never recover. The favourable circumstances are, absence of fever and oppression, free expectoration, and facility of vomiting. The disease terminates by the gradual wasting of *strength*, or runs into chronic affections, as consumption, asthma, hydrothorax and hydrocephalus, or suddenly, by apoplexy, or suffocation, from spasm of the glottis.

Touching the origin of pertussis, there is considerable doubt. It is supposed to depend on a specific contagion, which commonly affects persons only once. To this, however, there are many exceptions, several cases of which have come under our own observations. While it is pretty generally admitted, that the disease proceeds from contagion, there are not wanting some writers, who maintain, that it occasionally, at least, prevails as an epidemic, and hence must originate in a more common source. In relation to this controverted point, the facts seem not sufficiently numerous or well authenticated, to warrant any very positive conclusion.

Yet, we confess, that we are inclined to view it as dependent on causes of a more general and pervading influence than contagion. That it does, in some instances so arise, seems quite certain. It is expressly stated by Willis, who we have seen was the first to describe it, to be an epidemic, occurring most commonly in spring and autumn. By Hoffman, it is said to have spread in Berlin to a great extent in the same way. In the tenth volume of the Medical Repository of New-York, Dr. Willey gives an account of the disease having suddenly broken out in Block Island, and widely prevailed, without the inhabitants of the place having had any intercourse with an infected source. It is a rule, to which the exceptions are few, and perhaps none, that where a disease can be traced to atmospherical vitiation it does not prove contagious. Nature indeed, can hardly employ two such opposite causes to produce the same effect.

Entertaining the conviction, that the complaint is generated by specific contagion, we have at all events neglected, in a very great degree, to look for other causes of its production. Yet on the whole, in the present state of our intelligence, it will be most prudent to proceed in actual practice, under the impression of the contagious nature of the disease.

Consulting the various authorities on pertussis, we shall find that there is much diversity of sentiment in reference to its pathology. As respects its seat, it is by some placed in the air vessels of the lungs, either at their minute extremities, or larger ramifications. It has too been located in the larynx and the pharynx. By others, it is alleged with no little probability, to be primarily a complaint of the alimentary canal, with which some part of the pulmonary apparatus sympathises.

Nor has there been less controversy concerning the precise nature of the diseased action. By one set of pathologists, it is held to be purely spasmodic, while another, of equal weight of character insist, that it exhibits all the phenomena of active inflammation. The fact is, that till lately, we were very imperfectly instructed as to this disease. Dissections appear to have been rarely made in it, and hence as to its exact seat or nature, we had little else, except speculation and conjecture. As already intimated, the subject, however, has been at last taken up by our author, who, to a collection of all that was previously ascertained, has added much, the result of his own inquiries and observations. This is not the least interesting part of his work, and we shall cite it.

" It seems pretty clearly proved," says he, " that chincough is in all cases an inflammatory disease, and that its chief seat is in the mucous membrane of the larynx, trachea, bronchiæ, and air cells.

" When it is mild it may run its course, and cease spontaneously, without disturbing very materially the other functions of the body, or even the functions of that very membrane where it is seated.

" In some cases the inflammation, in its acute stage, is so severe and extensive in the mucous membrane, as to obstruct, if not wholly prevent, the objects of respiration, and thus proves fatal. Such patients generally die convulsed.

" In other instances the inflammation proceeds to a more advanced stage, producing such a profuse and altered secretion of mucus, as to plug up the air cells and bronchiæ, and thus prove fatal by preventing the access of the air.

" Sometimes the inflammation, leaving the mucous membrane, extends to the deeper seated parts, and proves fatal in the form of ordinary pneumonia.

" In other instances, the pneumonic inflammation runs on to suppuration, and abscesses are found in different parts of the substance of the lungs.

" Sometimes the inflammation leaving the mucous membrane, and extending to the deeper seated parts, excites tubercles, and the patient dies, with all the symptoms of pulmonary consumption.

" There may be other ways in which the disease terminates, but these at least seem to be ascertained from dissections. On the whole I am disposed to conclude, that whenever chincough proves either dangerous or fatal, it is by the degree of inflammation, in the natural seat of the disease, or by that inflammation extending, or being translated to other parts."

Notwithstanding the more correct light which has been thus shed on hooping cough, little new is proposed in the management of it. Long since, we had abundance of remedies, and all that seemed to be required was such a knowledge of its pathology as to render the application of them appropriate and decided. It has hitherto been very generally complained, that the practice of almost every one in this case is characterized by empiricism, or the want of those settled principles which guide us in most other diseases.

Two leading indications obviously present themselves in hooping cough. These are to subdue, in the first place, the violence of the disease, and secondly, to overcome the habit of perverted, or wrong association, by which it is manifestly kept up and continued, after the cause producing it is worn out or exhausted.

Taught by the evidence which has been revealed by dissections, as well as from the prominent symptoms of the case, we can hardly refuse our assent to the propriety of meeting the first stage of the complaint with evacuations: yet these are to be regulated by a sound discretion, and a proper adaption of the means to the state and circumstances of each particular case. The patient, whether a child or adult, being robust and florid, with a febrile pulse, and pulmonary oppression, must be bled.

It is, independently of other circumstances, especially demanded in many instances by the interrupted circulation of the lungs, and affords much relief. Exactly as in other diseases the bleeding is to be repeated should the necessity for it continue, or return subsequently, in the progress of the case. Even in Europe, where the lancet is so sparingly employed, comparatively, this practice is commended and generally pursued.*

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* Willis, who we have before stated as having first described pertussis, chiefly relies in the cure of it on bleeding, vomiting, purging, and blistering. This ap-

[illegible] ile thus depleting, evacuations of the alimentary [illegible] e not to be neglected—and which may be made by emet[illegible] or cathartics, according to circumstances. The former are chiefly applicable to children, and where the attack is violent [illegible] nd the oppression great, must be repeated daily, and [illegible] mes twice a day. To keep up the impression on th[illegible] nach, small doses of antimony or squills, or ipecacu[illegible] should [illegible] intervals.

[illegible] writers [illegible] h, or several of them at [illegible] ell with a [illegible] mphasis on the tendency to constipation, as [illegible] tiation in the contents of the bowels which [illegible] stages of the complaint. It is this, with some oth[illegible] erations, which has *led to* the notion of the proxi[illegible] of the disease being in the alimentary canal. But [illegible] apart, we are called upon to remove such a c[illegible] the bowels. The milder laxatives, though recomm[illegible] by some, do not answer half so well as calomel. Distinct from its purgative effects, this medicine would seem to exercise in all diseases, and strikingly in hooping cough, some other power. Certain it is, that active evacuations by calomel have, in a greater or less degree, the effect of breaking down the force of this disease, and to abridge its career. Confiding in its efficacy, it is an ancient practice of this city, to commence the treatment of the case with the mercurial purge repeated every two or three days. Whatever view we may adopt of its mode of action, or the measure of its utility, there can be no dispute as to the propriety of keeping the bowels open, and that calomel for this purpose, is to be selected.

During the attempt to make an impression on the disease by the general treatment pointed out, we are not wholly to lose sight of some local remedies. Congestions of the lungs are apt to take place in hooping cough, which to remove, as well as the topical inflammation known to exist, blisters

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pears, too, to have been the general practice of the age, and particularly of Sydenham; besides which we have, among many inferior names; those of Astruc, Huxam, Hoffman, Sauvages, Hillary, Bisset, Home, Lettsom, &c. in favour of this course of treament.

are found decidedly advantageous. As tending to the same end, we may also resort to leeches or cups, and especially where the lungs appear to suffer *extremely* from either of these causes.

In the use of the preceding remedies, we wish it to be understood, that the case we have described is a very strong one, exacting to a considerable extent, the depletory measures. As the disease more generally presents itself, venesection is not at all demanded, or scarcely any other remedy, than occasionally an emetic or purgative, with the interposition of nauseants, to promote expectoration.

Being somewhat subdued, pertussis requires in some degree a change of treatment. In the fluctuations of our practice in this case, the means introduced for its cure, have been exceedingly numerous and diversified. No one article at present seems to have a larger share of confidence, in regular as well as popular practice, than the *fixed alkalies.* Though, employed at an earlier date, by some German practitioners, the credit of having fully established their efficacy is accorded, and we think with justice, to Dr. Richard Pearson, of London. His prescription is annexed.*

At the moment that this preparation was greatly used in regular practice, a combination of the carbonate of potash and cochineal was put forth, we know not by whom, which has gained such universal confidence, as nearly to supplant all other remedies. The formula will be found below.†

The experience we have had with the alkalies is suffi-

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* ℞	Carb. Sod.	gr. iii.
	Vin. Ipecac.	gt. v.
	Tinct. Theb.	gt. i.
	Aq. font.	ʒ i.

This is to be given every three hours, to a child one year old.

† ℞	Carb. Potass.	ʒ j.
	Cochin.	gr. x.
	Aq. font.	℥ iv.
	Sacc. alb.	gr. vj.

The dose a tea spoonfull every two or three hours for a child a year old.

cient to enable us to pronounce with some certainty on their efficacy, and we do not at all doubt it. To the same purport we have the concurrent evidence of many respectable physicians, and the popular voice strongly expressed. That, however, the full effect of the medicine may be attained, it should be given in much larger doses than ordered in the preceding formulæ.

Nor are the powers of the alkalies limited alone to pertussis. On the contrary we are persuaded that they will be found beneficial in all cases where a mild expectorant, or cough medicine, is demanded. Of the comparative merits of the two, we cannot judge. Lately we have got into the habit of prescribing the potash, and such appears to be the case with the other medical men of this city, though we are not aware that this preference rests on any solid grounds.

The modus operandi of the alkalies in these affections is not very intelligible. It is alleged that they do good by neutralizing or correcting the acid sordes of the alimentary canal. That accumulations of foul, sour matter do exist in pertussis, and that the lungs will be sympathetically affected in consequence of gastric irritation, are facts as well attested as any in pathology.

Coughs of an inveterate character, even running on to consumption, we have sometimes met with, which could be distinctly traced to this source. It is not, therefore, altogether improbable, that such may be the modus operandi of these substances. Yet we do not perceive the necessity of resorting to a chemical solution of the problem. Contrary to common opinion, the alkalies really exert a pretty decisive agency on the system. This is illustrated in several diseases, and particularly in those of a periodical nature. It is well ascertained that a few grains of the carbonate of soda, added to a very small portion of Peruvian bark and Virginia snake root, constitute one of the most efficacious remedies in ague and fever. By some of the German writers we are also told, that they are not without use in the neuroses, and even in epilepsy. As the alkalies act in these cases, so most likely do they in pertussis by a strong and peculiar impression made on the stomach and extended to the lungs by consent of parts.

Nearly under the same circumstances of the disease are the narcotics and antispasmodics directed, and among these opium claims our first notice. Evacuations to a proper extent having been premised, its use as a palliative of the more vehement symptoms is fully sanctioned. It is, indeed, alleged to evince much greater powers over the disease, though I have never been able to discern any such effects. Exhibited alone, or as an ingredient in cough mixtures, it will calm pectoral irritation, postpone or mitigate the paroxysms, and so much only have we a right, in our opinion, to expect from opiates. Of the cicuta, much was, at one time said. Coming from Butter, whose authority was high in the diseases of children, it acquired immense notoriety, and was greatly extolled. Experience, however, more enlarged, led to a gradual depreciation of the article, till finally it has become entirely exploded. This is also pretty much the fate of the nightshade, henbane, digitalis, belladona, and other members of the same family. Considering its utility in asthma, we ought, perhaps, to make an exception in favour of stramonium, in the cases of adults, who might smoke it advantageously as a preventive of the paroxysm: but this is a mere conjecture, to be tested by future experience. We have heard too, lately, of the revival of belladona, with what success we have not ascertained, and the same may be said of the lactuca virosa.

During the prevalence of the pathology, which ascribed the disease to spasm, antispasmodics were the chief remedies. Of this class, the whole were tried in succession, and particularly the castor, musk and assafœtida. Of the former, we are entirely ignorant, having never seen it prescribed. Cullen, however, tells us, that it is of no value. It is true, he passes the same sentence of condemnation on musk, with which we cannot at all coincide. Could we, indeed, so prepare it as to render its exhibition more easy, we hardly know a remedy of greater importance. Even this objection does not apply to persons beyond childhood. But whatever though it is not always the case, as children, on the contrary, sometimes acquire even a relish for it.

Next, we are to point out the measures appropriate to the second indication in pertussis.

To interrupt the trains of morbid association, all the tonics have been directed, and especially such as are supposed to have the effect of subduing paroxysmal tendencies. The Peruvian bark was of course placed at the head of this class, and is much celebrated. Cullen bestows on it unqualified praise, considering it by far the most certain means, and even says, when given in sufficient quantity, he has seldom seen it fail of speedily putting an end to the disease. It is reasonable to suppose, that the bark might be useful, though it must be confessed that we have not witnessed such striking results from it, and on account of the difficulty of getting children to take it with regularity, it is now rarely prescribed by us, in their cases.

Exactly on the same principle, the arsenical solution is employed, and we have the strong testimony of Simmons and Ferriar in support of it. Each of these writers goes so far as to declare, that it is the only medicine deserving of much confidence. Whether it be so we are not able to determine.

The sacch. saturni has been added to the stock of new remedies in pertussis, and the praise of doing it is claimed by Rees, and generally allowed. In looking over, however the works of Sauvages the nosologist, we find that it was previously noticed by him. After enumerating various other remedies, he proceeds to observe, " Nec-non Sacch. Saturn. ad grana duo, tria."

Could we credit one half of what Rees has affirmed of its efficacy, we might be content, and stop further inquiries. Of its precise degree of utility, we know not. Nor are we able to say more of the nitrate of silver, so highly extolled within a few years by Jones.

Though now somewhat obsolete, the white vitriol, we suspect to have properties of more than ordinary value. It stage of the disease, we have already spoken, and not less advantageously may external applications be resorted to, at this conjuncture. Liniments of an irritating nature, such as the volatile, or the camphorated, or spirit of turpentine, mixed with olive oil, and above all, the juice of garlick rubbed along the vertebral column, are acknowledged to do good.* It is well understood, how much the action of the lungs is dependent on a nervous influence from the spinal marrow, and it is on this principle, perhaps, the efficacy of such applications is to be explained.

It remains for us only to make a few remarks on the appropriate regimen in the disease, and which we deem the more important, as it is a part of the treatment exceedingly neglected. Dissections have now established, independently of other evidence, the inflammatory nature, in many instances at least, of pertussis. It is therefore obviously proper, under such circumstances, that the antiphlogistic course in the commencement, is to be pursued in all its details.

Of the decided and sovereign efficacy of a change of residence, more particularly to the country, and even of a frequent exposure to fresh air, every one is so fully persuaded, that the remedy is abused by its general and indiscriminate adoption. It is by no means uncommon to see children thus exposed, in the coldest and most inclement seasons, and this sometimes, by the orders of the attending physician. Nothing can be more pernicious and ill-judged. As much as in the ordinary inflammatory catarrh should we, on the contrary, guard against cold, by keeping the patient in a room duly warmed. The lungs in this case are either inflamed, or peculiarly susceptible of inflammation, so that the slighest exposure brings on, renews, or violently exasperates the attack. Catarrh or active pneumony, superinduced on pertussis, constitutes a most formidable case, and most requently is the way in which the disease proves obstinate [illegible] fatal. But the inflammatory stage having passed over, and he weather being mild, then with incomparable benefit may the child be walked or rode out daily. Completely to eradicate the complaint, the patient should be carried into the country, and especially to the sea shore.

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* By Struve, a German writer, a mixture composed of one scruple of emetic tartar dissolved in two ounces of water, to which is added one ounce of tincture of cantharides, is directed to be rubbed in small quantities, every two or three hours, over the region of the stomach, and asserts that the greatest advantage results from the application.

Certain m[illegible]ations [illegible] a[illegible]osphere have been thought useful. Ch[illegible] such [illegible] impression, "have been taken down [illegible] deep mines, into coal-pits, into tan-yards, soap-works, [illegible]e-w[illegible] hether any advantage has resulted fron [illegible]ich [illegible]ite doubtful. The fumigations of tar or t[illegible] been likewise *employed*, though we app[illegible]ot much more success.

As stated [illegible]ich is the treat[illegible]t which, perhaps, at present is most [illegible]oved in [illegible]is. Of the remedies enumerated some, are dese[illegible] not much regard. But in morals, it is often as importan[illegible] point out evil as to indicate good—and the same holds as respects the practice of physic. Nothing surely is more useful to an inexperienced practitioner, when he has pressed on his attention a vast crowd of remedies, each probably as highly extolled, to have told him their comparative efficacy. It is in this way, too, that we are enabled to avoid evil and pursue good.

We have been induced to enter thus fully into the pathology and treatment of pertussis, from the importance we attach to it. Excepting cholera infantum, there is, perhaps, no one of the infantile diseases, generally, less understood, or which in this country is more destructive of life, or makes a wider inroad into domestic happiness.

Art. XIII. *An Account of the late Dr. John Morgan. Delivered before the Trustees and Students of Medicine in the College of Philadelphia, on the 2d of November,* 1789, *by* Benjamin Rush, M. D.

It would be unpardonable to enter upon the duties of the Chair of the late Professor of the Theory and Practice of Medicine, without paying a tribute of respect to his memory.

Dr. John Morgan, whose place I have been called upon to fill, was born in the city of Philadelphia. He discovered in early life a strong propensity for learning, and an uncommon application to books. He acquired the rudiments of his classical learning at the Rev. Dr. Finley's academy in Nottingham, and finished his studies in this college under the present provost and the late Rev. Dr. Allison. In both of these seminaries he acquired the esteem and affection of his preceptors, by his singular diligence and proficiency in his studies. In the year 1757 he was admitted to the first literary honours that were conferred by the college of Philadelphia.

During the last years of his attendance upon the college, he began the study of physic under the direction of Dr. John Redman, of this city. His conduct, as an apprentice, was such as gained him the esteem and confidence of his master, and the affections of all his patients. After he had finished his studies under Dr. Redman, he entered into the service of his country, as a surgeon and lieutenant in the provincial troops of Pennsylvania, in the last war which Britain and America carried on against the French nation. As a surgeon, in which capacity only he acted in the army, he acquired both knowledge and reputation. He was respected by the officers, and beloved by the soldiers of the army; and so great were his diligence and humanity in attending the sick and wounded, who were the subjects of his care, that I well remember to have heard it said, "that if it were possible for any man to merit Heaven by his good works, Dr. Morgan would deserve it for his faithful attendance upon his patients."

In the year 1760 he left the army, and sailed for Europe, with a view of prosecuting his studies in medicine.

He attended the lectures and dissections of the late celebrated Dr. William Hunter, and afterwards spent two years in attending the lectures of the professors in Edinburgh. Here, both the Monroes, Cullen, Rutherford, Whyte and Hope, were his masters, with each of whom he lived in the most familiar intercourse, and all of whom spoke of him with affection and respect. At the end of two years he published an elaborate Thesis upon the Formation of Pus, and after publicly defending it, was admitted to the honour of Doctor of Medicine in the University.

From Edinburgh he went to Paris, where he spent a winter in attending the anatomical lectures and dissections of Mr. Sue. In this city he injected a kidney in so curious and elegant a manner, that it procured his admission into the Academy of Surgery in Paris. While on the continent of Europe he visited Holland and Italy. In both these countries he was introduced to the first medical and literary characters. He spent several hours in company with Voltaire at Geneva, and he had the honour of a long conference with the celebrated Morgagni at Padua, when he was in the eightieth year of his age. This venerable physician, who was the light and ornament of two or three successive generations of physicians, was so pleased with the doctor, that he claimed kindred with him, from the resemblance of their names, and on the blank leaf of a copy of his works, which he presented to him, he inscribed with his own hand the following words: "*Affini suo, medico præclarissimo, Johanni Morgan, donat auctor.*" Upon the doctor's return to London he was elected a fellow of the Royal Society. He was likewise admitted as a licentiate of the College of Physicians in London, and a member of the College of Physicians in Edinburgh.

It was during his absence from home that he concerted with Dr. Shippen the plan of establishing a Medical School in this city. He returned to Philadelphia in the year 1765, loaded with literary honours, and was received with open arms by his fellow-citizens. They felt an interest in him, for having advanced in every part of Europe the honour of the American name. Immediately after his arrival, he was elected Professor of the Theory and Practice of Medicine, and delivered soon afterwards, at a public commencement, his plan for connecting a Medical School with the College of this city. This discourse was composed with taste and judgment, and contained many of the true principles of liberal medical science.

In the year 1769, he had the pleasure of seeing the first fruits of his labours for the advancement of medicine. Five young gentlemen received in that year from the hands of the present provost the first honours in medicine that ever were conferred in America.

The historian who shall hereafter relate the progress of medical science in America, will be deficient in candour and justice if he does not connect the name of Dr. Morgan with that auspicious æra, in which medicine was first taught and studied as a science in this country. But the zeal of Dr. Morgan was not confined to the advancement of medical science alone. He had an active hand in the establishment of the American Philosophical Society, and he undertook in the year 1773, a voyage to Jamaica, on purpose to solicit benefactions for the advancement of general literature in the college.

He possessed an uncommon capacity for acquiring knowledge. His memory was extensive and accurate; he was intimately acquainted with the Latin and Greek classics. He had read much in medicine. In all his pursuits he was persevering and indefatigable. He was capable of friendship, and in his intercourse with his patients discovered the most amiable and exemplary tenderness. I never knew a person who had been attended by him, that did not speak of his sympathy and attention with gratitude and respect. Such was the man who once filled the Chair of the Theory and

Practice of Medicine in our College. He is now no more. His remains now sleep in the silent grave—but not so his virtuous actions. Every act of benevolence which he performed, every public spirited enterprise which he planned or executed, and every tear of sympathy which he shed, are faithfully recorded, and shall be preserved forever.

TO READERS AND CORRESPONDENTS.

To the initial article of the present number, we wish particularly to call attention. It is written in the best manner of its venerable author, and discusses a most important subject, with all the force of dialectic precision, and the fascinations of eloquence.—The earliest of his productions, this Essay, can now scarcely be read without the mournful reflection, how much might have been done for Medicine, and the interests of elegant letters, had he been cast on a wider and more advantageous theatre, for the incitement and display of the powers and resources of his large and finely cultivated mind.

We have had transmitted to us, a very elaborate Review of Dr. Hosack's late Pamphlet on Febrile Contagion. Though the analysis is acute, and the style sufficiently pure, we cannot on the whole, approve of it, and therefore will return the manuscript whenever the author shall indicate his address. We wish it to be clearly understood, that the object of this Journal is to endeavour to elevate the character of medicine in the United States, and so long as it may be under our control, its pages shall never be sullied by personalities, nor prostituted to low and unworthy purposes.

Differing, as we do, from Dr. Hosack, in our views of the origin of yellow fever, we can never cease to applaud the talent, industry and success, with which he has investigated the subject. To him we think is especially due, the credit of establishing the fact of the insusceptibility of the system to a repetition of an attack of the disease, than which, we know of none more curious in pathology, or important in some of its practical tendencies.

The time we hoped had arrived, when the subject of yellow fever, once so provocative of bad passions, might be coolly and philosophically discussed. Certain it is, that in no other temper can truth be elicited, and surely there was never an occasion in which the united and harmonious exertions of medical men was more imperiously demanded.

Against the charge so gravely preferred against him by the author of the review, and also in a publication we have received from New-York, of having concealed the existence of yellow fever in that city, during the last season, we must protest. It is to us utterly incredible, that a man of his elevated character and station, should be guilty of an act of this sort, and for which, no adequate motive in our opinion is assigned. That a malignant fever did prevail to a very considerable extent in Bancker Street, seems quite certain. But we deem it not improbable, as is alleged by Dr. Hosack, that it was of a typhoid character, very distinct from our summer pestilence. To this conclusion we are led, independently of our respect for his authority, by the recollection that at the same time, a similar disease existed to a very considerable extent in our own city. That such was the case, no medical man among us denies.

It is altogether an interesting fact, and one which militates strongly against the common doctrine of the schools, which denies the co-existence of two epidemics in the same place, that while yellow fever spread along our wharves, we had in a remote and opposite direction another species of disease *widely* prevailing. Yet, though we enter the lists in defence of Dr. Hosack on these points, we do most entirely concur in the general sentiment of disapprobation of the proceedings of the Board of Health of New-York, in relation to this city. Every one of their measures on the occasion, seems to have been dictated by a narrow and illiberal spirit, growing out of notions of contagion, we think, wholly unfounded. Conduct like this, must inevitably be productive of much acrimony of feeling, and cannot fail ultimately to lead to acts of retaliation—at a season too, when there is a call for the fullest extension of our benevolent and friendly dispositions. The habitual distrust of the candour and fair dealing of the Board of Health of this city, to say the least of it, was eminently discourteous, and in every respect unwarrantable. That Board was composed of some of the most respectable of our citizens, and had as its president a physician, who from weight of character, general intelligence, and rectitude of understanding, ought to have given to its acts a more than ordinary degree of authority and credibility.

If the official statements of such a Board are to be rudely rejected, as false, and even the veracity of its members impugned, there must be an end to all amity and intercourse between our cities, and in times of the severest trial and difficulty, each one in hostile attitude, will be left to itself, and in place of a generous competition in the cause of humanity, we shall have presented the spectacle, of an ignoble struggle, which shall do to the other most harm.

It affords us very sincere satisfaction, to be able to assert from a source, which we consider authentic, that though the Board of Health of New-York, were guided in their general restrictive policy by the counsel of Dr. Hosack, he had no participation in those offensive measures, on which we have thought it our duty to animadvert, and entirely persuaded are we, from an intimate acquaintance with him, that there is no individual out of Philadelphia who has for it feelings of more kindness, partiality, and attachment, or who takes a livelier interest in its prosperity and welfare.

By one of our correspondents, on whom we can implicitly rely, we are informed, that the medical class in the University of Transylvania, this season, amounts to *ninety students.* To us, this intelligence is exceedingly gratifying. Embracing in its professors some of the best talents of our country, and *located* among a people, prompt to the perception of its utility, as well as liberal to sustain it, there is no reason why this school should not succeed. Of this our friends who are embarked in the enterprise may be persuaded, that our good wishes are with them, and that so far as this Journal, or our exertions in any shape, may be thought capable of promoting its fortunes, they shall be employed.

Not without regret, we have noticed the indications of more than one rival institution, starting up in the western country. There is, indeed, at present, a pruriency of this sort, pervading the United States, which if not timely restrained, must be productive of consequences fatal to the hopes which are indulged, of elevating the character and condition of medicine among us. Why, in particular, do the medical men of the west, desire to dilute, by such diffusion, the talent which properly concentrated, promises the most useful and brilliant results? Be it their ambition, on the contrary, to sacrifice petty interests, and local prejudices, and harmoniously co-operate, to rear up one great school, which may be creditable to themselves, beneficial to the profession, and honourable to that section of the country. In an enterprise of this nature, we should delight to find the name of Drake associated, than which, there is scarcely one more advantageously known in every part of the United States.

It has been suggested, by one of our friends, that the article on the "Nourishment of the Fœtus," in our last number, contains a passage somewhat obscure, and which, indeed, apparently involves a contradiction. After stating, that the nutritive fluid is *secreted* into the placental cells, by the *uterine arteries*, it is declared, in the very next page, that the *epigastric artery* is the *immediate instrument* by which the *secretion* is performed, &c. Detached, the passage does seem amenable to this criticism, and it must be allowed, is not very lucidly expressed, owing to the hasty abridgment for the press, of a much more copious disquisition on the subject. The sense, however, is sufficiently explained by the context. It is obviously the meaning of the author, that the *epigastric*, from its anastamosis with the *internal mammary* artery, forms, mainly, the sympathetic connexion between the breasts and womb, and is the medium through which the faculty as well as the elements of secretion are transferred to the latter organ, in a state of pregnancy, though to speak with perfect precision, it is the uterine vessels which directly perform the office of elaborating the fluid.

Whether the author of the paper is entitled to the credit of having originated the hypothesis of the fœtus being sustained by lymphatic absorption, is a matter with him of little concern. Certain it is, however, that the elder Monro is not at all entitled to it—nor indeed is it claimed for him in the recent anatomical work of his grandson, who gives it to Schreder. In the time of Monro, absorption was supposed to be the office of the veins, and his notion relative to fœtal nourishment, is, that the *radicals of the umbilical vein take up only the serous portion of the maternal blood, as the nutriment of the fœtus, having orifices too small for the admission of the red globules.* Many years subsequently to the publication of Monro's paper on this physiological question, the two Hunters confuted the then prevalent opinions of venous absorption, &c. &c.

With the highest possible satisfaction, we announce the appointment of Dr. WILLIAM E. HORNER, as adjunct Professor of Anatomy in the University of Pennsylvania. The entire unanimity with which this honour was conferred, shows the sense entertained by the Trustees of his talents and acquirements, and may be considered as a just reward for his long and meritorious services to the school.

Professor GIBSON is preparing for the press a work on the Elements of Surgery. It is to appear early next summer—and we have no doubt, from his habit of accurate and erudite writing, will prove an important accession to the stock of our native medical literature.

ERRATA.

In page 304, 12th line from the top, after the word "derangement," insert, "*where the bleeding is large.*"

Page 383, line 20 from the bottom, insert the word "*which,*" between the words "decoction" and "may."

Next line below, for "may," read "*will.*"

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